OPTOMETRIC · EXTENSION · PROGRAM
OEP
1928

PSYCHOLOGICAL OPTICS

QP475
R45
Vol. V

LIBRARY OF
L. E. APPLE

P S Y C H O L O G I C A L O P T I C S

V I S U A L P S Y C H O L O G Y

by

Samuel Renshaw, M. A., Ph. D.

Book V - Series 19-20-21

A series of papers released by the Optometric Extension Program to its membership 1958-1961

Compiled and Published by
Optometric Extension Program
Duncan, Oklahoma
1962

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1958
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

OPTOMETRIC EXTENSION PROGRAM 1928

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshow, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

October - 1958

Series 19 No. 1

In the preceding series we have reviewed the contributions of Berkeley, Scheiner, Mach, Wheatstone, Brewster, and the development of the basic concepts of binocular, tridimensional seeing; then we turned to George Stratton and the problem of the inverted retinal 'image,' and ended the series with some problems arising out of the important, if often neglected, concept of visual apparent movement phenomena.

Then a turning point in our cultural history came with the brilliant observations and constructive thinking of Max Wertheimer, and of his colleagues.

Wertheimer simply could not reconcile the then current over-simplicity of the stimulus-response atomism to make it give any kind of satisfactory explanation of the observed facts seen in his experiments on the visual perception of motion. So he had to take a different attack. This was in the early years of the present century.

In previous papers I have pointed out that in the history of the biological sciences there has always been a dilemma. One could espouse either the cell theory (dating from about 1865) which held that cells were the basic building materials and that one could only know the organism and its functions by studying its constituent cells and their functions and properties. Or, one could take the opposing view, older in fact, the organismal view, ably expressed by Du Bary, the German botanist, who said that "plants build cells, not cells plants."

When Wertheimer turned, in psychology, to an organismal point of view for a more satisfactory account not only of visual apparent movement, but of perception, learning, memory, thinking and problem solution, animal behavior and even abnormal phenomena, and in so doing moved away diametrically from the then popular and dominating practice of 'explaining' human behavior and experiences by analyzing them into patterns of sensations, simple images and simple feelings, he was traveling a road whose trail had been blazed, but unfortunately the pioneers had left no clear guideposts.

This statement, please understand, is in no sense intended to take away anything of the importance of Wertheimer's extension of wholism or organismal theory into psychology. I wish merely to point out that the movement in this direction was, between about 1895 and 1915, "in the air."

The German philosophers von Ehrenfels, Husserl, and Meinong had been much concerned with the problem of 'form quality.' They saw that just as a melody was something more than a mere succession of single tones, or that a face was more than just eyes, nose, cheeks, etc., - these things had something, some properties which were not explicable in terms of anything known or derivable directly from the sensory inputs from the 'parts.'

There was much "in the air" about this time. In 1903 for instance Cornelius objected to atomism and to the dire necessity of inventing a hypothetical mechanism of association to unify the 'atoms' of the then current psychological description of experiences. In 1915 C.J. Herrick and Carl Rahn made in a paper a devastating criticism, never adequately answered, of sensation as anything real or defensable. In 1890 William James, in his famous Principles of Psychology, rejected the doctrine that space relations could be explained by the kind of 'special act' proposed by Hering, and about this same time von Kries and Becher could not find in the prevailing physiology of the time any satisfactory 'explanation' of space and form perception, learning, memory, thinking, etc.; and even

Wilhelm Wundt, the pioneer founder of the first laboratory of experimental psychology, struggling over the problem of how to account for emergent qualities in perception, seen by any competent observer, had to postulate what he called the process of 'creative synthesis' in addition to 'association' to extricate himself from the difficulty. This was purely stochastic.

As early as 1907 J.B.S. Haldane, the eminent British physiologist, studying the mechanism of respiration, proposed that the body is something more than the sum of its parts, and that the organism and its environment are one and inseparable. In 1910 Max Planck, the great originator of quantum mechanics in physics, gave a clear-cut exposition of the necessity of a field or organismal theory, not only in physics but in all divisions of science.

At about the same time C.M. Child, the Herricks, and their students at Chicago, in the developmental and genetic studies stressed, as did Sir Charles Sherrington in 1907, the totalizing and integrative function of the nervous system, just as Ernst Mach, many years before, had shown that seeing is done with <u>all</u> of our sensory and motor resources, not <u>just</u> the eyes and brain.

In 1926 the great physicist Alfred N. Whitehead published a book (Science and the Modern World) in which he said "The concrete and enduring entities are organsims, the plan of the whole influences the very characteristics of the subordinate parts which enter into it."

Chemists today show that <u>where</u> an atom is in the architecture of a <u>molecule</u> determines what operational effects it will have rather than anything physically true as to its mass, charge, movement, etc.

I merely wish to point out that the time was ripe, in the first couple of decades of this and the preceding century, for a forward step to be taken, as it was in all divisions of science, for a broader look at wholism, configurationism, field theory, the organismal theory.

Wertheimer, like the two princes of Seranar, was there at the right time and the right places and so started a new kind of approach in psychology in 1912.

But new things "may be constellations of profundity or stars made by duck's tracks in the soft mud of a pond," and it is small wonder that almost at once those committed to the prevailing 'older' atomism got busy with their criticism. The reward for Copernicus, when he courageously proposed that the world was round instead of flat, was that he was burned at stake!

It is quite easy to be a critic whether you have earned the right to be one or not. So even today there are those who will have no truck with field theory, in whatever form it may be expressed. But even so, we must look at their criticisms.

It is often said that field or gestalt theory is nothing but new wine in old bottles. Our previous paragraphs have shown that this was a movement, not the single notion of any single individual.

A more elaborate refutation of this criticism was made by Wheeler, Perkins and Bartley in the 1931 and 1933 volumes of the Psychological Review.

As to the problem of visual form perception it has been charged that Aristotle solved this problem first. But to him <u>form</u> was something which could be disembedded or disembodied from 'real' things, whatever 'real' means, but in gestalt theory <u>form</u> is something like a vector or tensor function and it inheres within the dynamic system and cannot be separated from it as an independent entity.

Professor Allport (Theories of Perception, Wiley, N.Y., 1955, p. 589) claims that field theory "runs into difficulties with the facts of brain physiology" and that it is "at odds, also, with some genetic and clinical observations," and that it does not "generalize convincingly to sensory dimensions, to certain aspects of constancy, to concrete meaning, or to the prevailing set or state of the perceiver."

Adrian has ably pointed out that there is no satisfactory neurophysiological 'explanation' of perception. R.W. Gerard has shown that brain physiology has not given any satisfactory account of how we remember, and may I add that each of Professor

Allport's criticisms can be turned as well upon his own wordy exposition in his book on how we perceive and remember things.

Professor Köhler has ably replied to these kinds of criticisms by showing that field theory is an open and not a closed system; that time and further work will inevitably bring changes but that the basic postulates will be changed little if at all and are here to stay.

The whole history of science is one of a healthy kind of controversy. The critics of evolution have been one driving force to cause biologists to work harder for its proof. If the problems of how we see form, space, size, position, distance, etc., can be solved by some kind of atomistic analysis rather than by the multidimensional calculus of the modern mathematics in field dynamics, both the privilege and the responsibility is yours and yours alone.

Let me give you a problem, if you are a 'classical' theorist. E.H. Land has recently shown that you can take two black and white transparencies through red and green filters, then project these through two colored filters superposed and "the colors of the objects in the resultant image are remarkably stable in that they are largely independent of what wavelength bands are used for projection and also largely independent of the relative brightness of the two projected beams, perceived color at a point in an image cannot be described by wavelength composition or relative brightness of the constituent primaries."

Now if you are sure of yourself and believe that the answer to psychological problems is to be found in physics and physiology, here is your golden opportunity. How can you explain Dr. Land's findings? That there is an answer as to all things which happen in nature no one can question. But finalistic thinking will not get us there.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright 1958 OEP Foundation Inc. (Nonprofit) Duncan, Okla.

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshow, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

November - 1958

Series 19 No. 2

One of the founders and developers of the field or configuration theory in psychology was born in 1887 -- Wolfgang Köhler. He came to psychology trained in mathematics and physics. After working with Wertheimer and Koffra, his professorship at Berlin received bad treatment from the Nazi's and so Köhler came to America in 1922-25 and has been at Swarthmore, Chicago, Princeton.

During World War I he was interned on the Island of Teneriffe, off the Coast of Africa, and during the four years there he made many experiments on learning, insight and problem solution by Chimpanzees -- major contributions to animal psychology. Equally important contributions to psychophysics and to vision have been made by Professor Köhler. He is at present President of the American Psychological Association and the recipient of the highest academic honors. Even those who differ with him in point of view and theory yet respect his genius.

One of the very highly important things Köhler has contributed applies to the concept of traces. About 1918 he became interested in the fact, known long before, that if you set about to measure the difference limen or threshold, that is the smallest difference of weight, brightness, loudness, extent, etc., that an observer can discriminate, you immediately are confronted by a most interesting and important problem. This is the matter of the negative time error.

Suppose I ask you to lift two weights -- A and B. Suppose B is somewhat heavier that A. If I give you 50 comparisons A then B, and 50 B then A, we will find that AB will yield a greater number of correct judgments than if I present them to you in the order BA. This is the negative time error. From the standpoint of pure logic, the time order should make no difference. But it does. Köhler studied the problem. In one of his experiments he wanted to find out if the error was also present when two equally loud clicks were presented and the observer was required to judge the relative loudness of the two. Here are his results:

Judgment	Interval in Seconds 1 1/2	3	4 1/2	6
Second louder	4.2	29.2	54.2	62.5
Second softer	62.5	50	25	8.3

Here the results show law and order. But if the trace left in the nervous impulses of the first stimulus are gradually weakened by metabolic processes, and this causes the larger negative time error with the longer intervals, then an alternative hypothesis is that traces (i.e.,hysteresis effects in the protoplasm of the nerve cells) near to each other in time become assimilated to each other, and the trace of the first excitation would tend to combine and "reinforce" that of the second. But here is our old friend the "all or none" law in neurology. So the question, extending to all memory trace systems, arises: What are the facts about trace interactions? Do they remain single processes or do they coalesce in the manner seen in other perceptual phenomena, following the principles proposed by Wertheimer for field structuring or organization: proximity, similarity, good continuation, closure, etc.?

It is important to note that the same relative values for the negative time error Köhler secured for sounds were also secured by Lauenstein for brightness discriminations. In both auditory and visual fields there was found a positive time

error with short intervals, and thereafter a time error, increasing with the length of the intervals, which was negative for the weaker ground surrounding the stimulus and positive for the stronger grounds.

After about a second the trace assimilates with the trace system of its background, in the vision experiments, and a progressive time error is produced, either positive or negative, according to the brightness and area of the ground.

Clearly, as the earlier pioneering work of Rubin on figure-ground dynamics had shown, figure intensity could only be expressed as some ratio of figure to ground and never as a constant brightness, loudness or heaviness of the "stimulus" alone.

It is not possible in the limits of this paper to detail all the experimental results bearing upon this problem. Suffice it to say that here were instances in which the classical concepts of psychophysics had to be revised and extended. Whether one liked it or not the worker was in field dynamics deeply and no matter how precisely the control of stimulus intensities, the factor of time had to be written into the equation as did the relation of the figure (i.e., the stimulus) to its ground. Here "ground" could include not only the stimulus surround, but as we shall see, all the vector quantities from the memory trace systems in addition.

Several investigators had observed, among them J. J. Gibson, that if in prolonged inspection one looked at slightly curved lines, the lines gradually became less curved. And when after such inspection and decrease of curvature, straight lines were shown in the location and orientation of the curves, the straight lines now appeared curved in the opposite direction. Delabarre saw this almost a third of a century earlier but dismissed the thing as an "illusion" and paid no attention to its theoretical significance, because then the major emphasis was upon the atomistic analysis of experiences. Any thing which did not fit the prevailing point of view was consequently disregarded.

Was this phenomenon, which Köhler later called figurial after effects, something unique and peculiar to vision? Did it work visually other than in the frontal horizontal plane?

When a line or rod was tilted toward or away from the observer either the vertical or horizontal, it appeared tilted in the opposite direction. Gibson observed that these after-effects were restricted fairly closely to the region of the inspection-figure, and that if only one eye was used a somewhat smaller after effect could be observed clearly in the corresponding part of the other eye's field. And strikingly enough long exposure times were not necessary for noticeable after-effects and often they would appear within 5 seconds and, importantly, some lasted for 24 hours and more recent experiments have greatly extended this curious "lasting power" of the after-effects, even to several days. But, strangely, some persons cannot get them at all!

When, blindfolded, the finger tips feel along the curved surface of a heavy piece of wire and then feel a straight wire in the same approximate orientation. The straight wire now feels slightly curved in the opposite direction (Köhler and Dinnerstein) so, figurial after effects are seen in vision, hearing, kinesthesis, and Köhler and Emery showed that the after effects in the visual third dimension were easier to set up and possessed other important facts about functioning in the z - axis which can not be disregarded.

Brain physiology has no explanation for these effects at present, although one probably will be found. But these studies have broadened the horizon of research in vision. Our debt to Wolfgang Köhler is great. It is a pleasure to acknowledge it here and to hope that all who engage in lens fitting and training may realize that in these phenomena may reside the germ of some very important future developments. It is regrettable the so few people, few laboratories, little subvention is being devoted to carrying on.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1958
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

OPTOMETRIC EXTENSION PROGRAM 1928

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshow, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

December - 1958

Series 19 No. 3

When one looks binocularly at a scene, some object or region of the total view becomes the figure, the salient part of the scene, set in the surrounding ground. The ground diminishes in clearness and sharpness in the periphery. It has other important characteristics which distinguish it from figure processes.

The figure is seen because it has a contour or boundary. Ernst Mach showed, many years ago, that contours form as second differentials of brightness or color, that is the rate of change of the rate of change has to be greater than a certain minimum. Figures form better in short than in long exposures. Craik showed that the dividing line in time between long and short was about 0.I. second and that in 'continuous' seeing the eye actually makes about 10 'snapshots' per second.

Graham had his observers look at the well known three angled 'propeller' inside a circle and fixate the center of the circle. It had long been known that the parts seen as the propeller remain only for some small finite period of time, then the figure becomes ground and the regions formerly seen as ground now become figure. The rate of the alternation behaves much like any learning function, that is the duration of the phases become less and less as time goes on. Graham varied the size of the angles of the propeller blades and discovered that as the angles were made smaller, within certain limits, the time of the figure phase was lengthened. If the blades and the interstices are colored differently it has long been known also that the change from figure to ground and vica versa brings with it a color change. The colored figure loses saturation when it becomes ground and regains it on the reverse change of phase.

These studies bring out two important facts: first that figures tend to be self-extinguishing. They only seem to last longer because we move the eyes, shift fixation, blink, etc. Second, figure stability (sometimes referred to as 'hard' and 'soft' figures) depends upon contour formation, and this is linked with the ratio or relative size of the figure to its ground. McFadden, in our laboratory, showed that this ratio is an important determinant of the measure of visual acuity, and I have proposed that it is the most probable answer to the size change in stereopsis effected by decentration of the two images (J.O.S.A., Nov. 1958).

Köhler showed a number of years ago that in reversable figures the figure-ground relation will be reversed when the figure process "has altered the medium beyond a critical stage; that the continued presence of any figure in a given location changes conditions for subsequent figure processes in the same region of the field; and that the most essential phase of figure after effects is a withdrawal of test objects from regions which have been affected by inspection of a figure."

Inspection here means centered fixation for a period of time sufficient to set up the change in the medium, that is in retino-cortical-motor-feedback system.

Köhler proposed that the neurophysiological mechanism of figural after effects is that of electro-tonus, which is a polarization effect. One of Köhler's very important contributions has been to show that figure phenomena are functions of direct currents in brain regions adjacent to the nerve nets active in conduction with alternating currents. For the polarization effects, brought about by inspection, in the medium to produce changes in the form or position of distance of test objects placed in the same region, he proposed the term satiation.

Köhler showed in his experiments that electro-tonic action is always self-limiting; that the effect appears after short inspection periods and soon reaches a maximum beyond which further inspection is ineffective. The degree of electrotonic action depends upon the intensity of the polarizing current, and this is proportional to the electromotive forces which maintain the flow. These EMF's of figure currents increase with the brightness difference between the figure and its ground, and it makes no difference whether the inspection figure is shown dark on a bright ground or bright on a dark ground, the EMF's are the same.

After prolonged flow of current in the tissues, varying from a few seconds to as much as 8 minutes, the electrotonus at the anodes, the anelectrotonus, becomes more and more perdominant and at this stage anelectrotonus provides an obstruction to all test currents irrespective of their direction. Köhler also found that if the number of cycles is not too high alternating currents may cause electrotonus.

Individuals differ widely in the amount and times of the figural after effects. Bales and Follansbee found them entirely absent in some subjects. The reason could be biochemical, and probably is, but at present we have no available information on this matter.

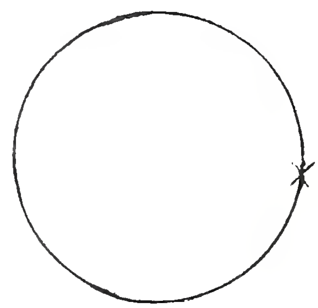
Since the monograph of Köhler and Wallach describing their many experiments on figural after effects is not likely to be immediately available to most readers of these papers, I should like to review some of them here because they provide interesting and important information as to the processes and mechanisms involved in the seeing of forms. They point the way research will probably have to go which leads to better means of controlling and rectifying by training certain disorders of functions in vision of great importance to all of us.

This view is unfortunately not shared by many workers in vision. For example a recent book on perception by a prominent psychologist devotes *two* pages to figural after effects and dismisses them as unimportant, *mirabile dictu*, because in spite of all we have pointed out previously in this and other papers of this series, "they do not coform to the neurophysiology of the brain"!

Köhler has ably refuted this kind of criticism in a paper "The Present Situation in Brain Physiology" in the American Psychologist, 1958, *13*, 150-154.

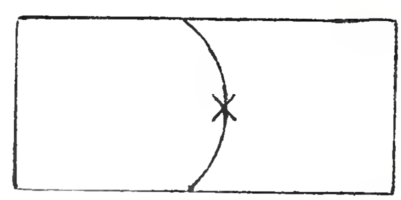
An inspection figure was shown, a circle with the fixation point at *x*.

I



The observers maintained steady fixation for periods of 40 seconds to 8 minutes. Then the test object was shown, a rectangle divided by a curve which was a segment of the circle with the fixation point in the same field position so that the curved line inside the rectangle was congruent with the corresponding part of the circle.

T



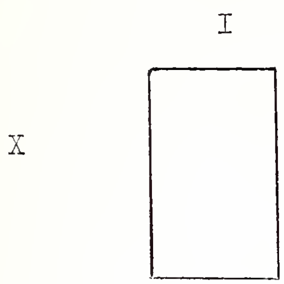
All observers saw the *right* side of the test rectangle as larger, and nearer in space. Köhler points out that under ordinary circumstances just the opposite depth effect is generally seen. Convex rather than concave areas are usually seen as figures. But after the inspection period the relation is inverted and the left part of the rectangle is seen smaller.

Obviously the space inside the inspection object, the circle, has been compressed and this effect could only be due to some

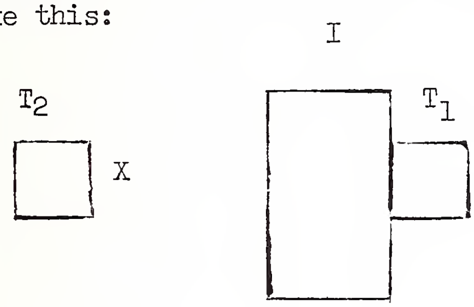
kind of alteration of the medium.

In the case of the rectangle partly falling within the circle, the question naturally arose: Does the effect stop at the boundary?

To test this an inspection figure was shown like this, the fixation spot being at x.

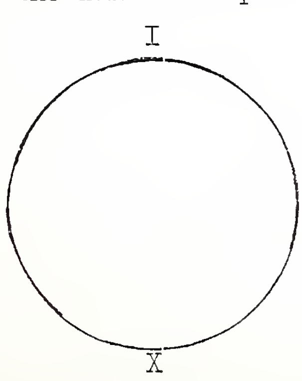


Then the test objects, two squares, were shown, such that the right square coincided with the right margin of the I figure, like this:

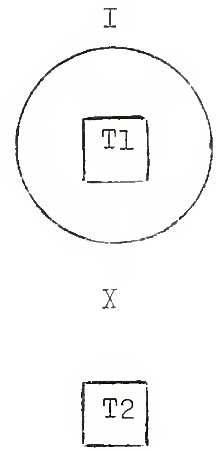


T_1 was seen smaller than T_2. It lies farther back in space, it had paler, grayer lines. So Köhler concluded that the after effect obviously extends beyond the area of the I object.

Suppose now the T object is shown where it has no point in common with the contour of the I figure. A circle was shown, x again being the fixation spot:



After this test objects were placed as follows:



T1 was seen smaller, farther back in space, the lines paler and grayer. The conclusion was that the after effect thus must pervade the whole interior of the I object.

The question then arose: Is the effect due to the amount of contour of the I object surrounding the T object at a given distance?

Two pairs of parallel bars were set up on the left and were made 25 per cent greater in contour than the square I object at the right, with the fixation spot between them. The two equal test squares were placed within the boundaries of the I figures. The T2 at the left was seen larger and nearer. The authors conclude that "the strength of the after effect depends upon the specific configuration in which the I contours are given in the neighborhood of the T object. Mere quantity of contour at a given distance is surely not the only decisive factor."

Critics have asserted that the use of simple drawings of circles, squares, parallel lines, etc., as I objects makes a special case of the experiments. But when Köhler used sponges, a tobacco pouch or a dollar bill as I objects, squares placed in the same position as the T objects showed the same figural after effects. More about this next time.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1958
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshow, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGIC OPTICS

January - 1959 Series 19 No. 4

Often in the history of science a discovery is made which profoundly influences subsequent thinking and developments in science. Numerous examples come to mind. One of these, and a very important one, was the contribution of Michael Faraday, field theory, which like Planck's quantum theory, reshaped the subsequent course of physics. Faraday showed that when any object is placed in a field there is a redistribution of field forces. This led to Kirchoff's laws relating to the flow of currents in complex electrical fields, for example. Instances could be multiplied greatly.

Is the work of Wolfgang Köhler and his co-workers one of these epochal events in the sciences which deal with visual processes? Has he pointed the way to some kind of satisfactory solution to the problems of the relations of the neural and biochemical processes of the body and our perceptual and memorial experiences? Can we accept the fundamental theorem of isomorphism that the <u>molar</u> processes of perceiving, understanding and creative thinking are nothing more than the physical <u>molecular</u> processes <u>in extension</u>?

If the answer is in the affirmative then are we committed to an out and out physical monism, as Mach proposed, and if so, can we reconcile such a view with our basic philosophic and ethical concepts?

Let us look further at Köhler's work for help in our attempts to answer these and similar questions.

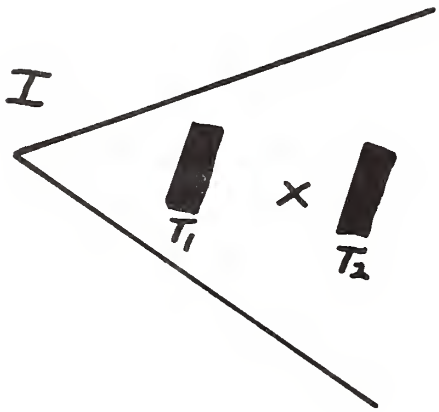
The visual field is a unitary medium. It is partly 'out there' and partly within the unit perceiving organism - the perceiver, self or ego - and this too is partly 'out there' and partly 'in here.' The visual field, said Köhler, is normally an anisotropic medium. That is to say that it is not homogeneous in spatial and temporal and sensitivity characteristics throughout its range. This medium, particularly that portion which can be localized in the brain, can be altered by polarization effects which are consequent to activity. Some of these effects are unstable and transitory; others resemble the steady states of physics and may extend in time to some approximation to permanency. These were called figural after effects.

Extensive experiments showed that figural after effects "are generally stronger in the lower half of the field and they also appear here earlier." This fact was found not to be true of the horizontal dimension, where displacements and distortions, due to the prior anelectrotonus of that part of the field, were always found to be symmetrical. Changes in the <u>position</u> of a field object were attributed to satiation effects. Changes in <u>size</u>, the experiments showed, did not work this way. To quote, "satiation should be maximal in the region of the outline or boundary; actually in this location the <u>size</u> of the test object seems to be much less reduced than is that of a smaller test object which, one would think, lies in a less affected area."

This fact was named the distance paradox, and the authors point out that "the distinction must be drawn between figural after effects and satiation since a test object that lies wholly in a region of maximal satiation need not show a strong after effect in every respect." "When a test object is removed from the region of maximal satiation we find those other symptoms weakening while the size effect grows" hence it follows that "the various symptoms which are caused by satiation are independent of one another." Generally speaking it was discovered that the size effect varies inversely with distance, within certain limits, from the

position of coincidence of the T object with the I object.

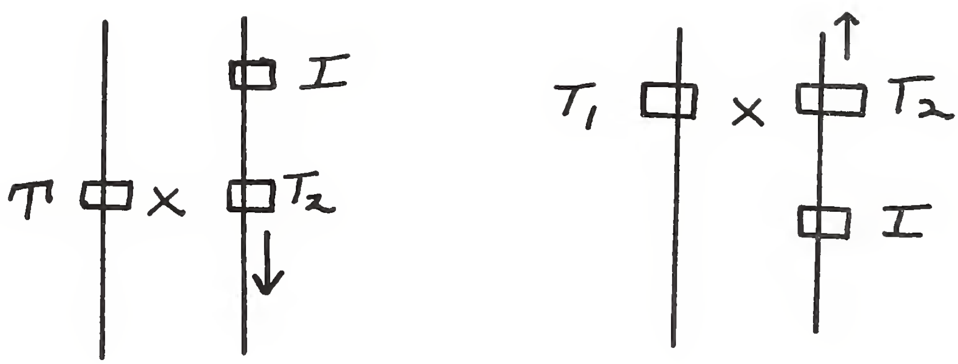
This can be more clearly understood by citing one of the experiments. In the classical Ponzo illusion the I or inspection figure is a black line angle of about 30° with the fixation point at X.



If the observer looks at the X for 40 seconds and the angular figure is removed and the two test objects, equal black rectangles, are shown equidistant from the fixation point, the right test object T2 is seen shorter, paler, and farther back in space. Thus the size effect is greater near the open end of the angle which comprises the I object. Logically the reverse should be true if the size effect were wholly due to the satiation effect, which should exist in a gradient which is least at the open end of the angle and greatest at or near the point of convergence of the two lines. This is why it had to be concluded that such effects as change in position, size, distance, tactual-kineasthetic, space, color, pitch, etc., as quoted in a previous paragraph are independent of one another.

This makes sense because if you take a dozen measures of what are called "visual skills" on a hundred persons and compute the interactions and intercorrelations, you will find a complete verification of this statement. It is rather a large assignment. I have done it and I learned two things from it: first, it takes a whale of a lot of time and labor; second, it shows that the names we give arbitrarily to visual functions may contain little or no information as to the actual operational processes involved.

Köhler and Emery showed that the figural after effect is stronger and comes up quicker in the third dimension than is the case of planar figures. The following diagram shows the plan of one of their simplest experiments:

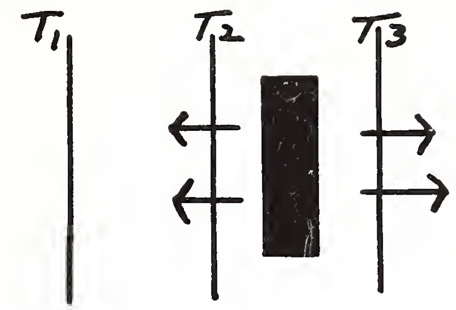


Importantly, they found that changing fixation after the I periods (40 seconds) can produce either a forward or back displacement. This being true, one may postulate that the ratio of the vertical and horizontal dimensions of the object to the solid stereo field angle is as important a determinant of the after effect as are the brain field properties. The forces within this field then must

coact with the energy distribution of the input signal.

Köhler has pointed out that some of these brain field effects are stable and long lasting, while others are temporary and transitory. It would follow then that both enhancing or reinforcing and inhibitory of diminishing effects are observed. Since objects seen in the vicinity of the boundaries of the satiation region tend to recede from that region it is logical to presume that the change of effect may be accounted for in this way when accommodation distance is changed from in front of or to the rear of the point of the inspection figure.

In another experiment by Köhler and Wallach an I figure, a black rectangle was shown with the fixation spot in its center. Afterward three parallel lines were shown as test objects in the positions shown in the diagram below:



Since the forces within the I figure are cetripetal, and beyond the contour of this figure direct currents will produce polarizing effects centrifugally from the I locus and since these effects will be opposite on the two lateral sides of the I figure and will diminish in a gradient which varies with the distance, T2 will move closer to T1 and farther from T3.

Since these effects may apply to any form it is justifiable to hypostatize that here may be one answer to the problems relating to the measurement of acuity.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshow, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

February - 1959 Series 19 No. 5

Because we have distance receptors and can make delayed reactions and have memories our behavior is not a sequential series of simple separate percepts and actions. The stream of behavior, in the broadest sense, the thing which makes me me and you you, is a unitary structural organization. It can, does and must transcend both spatial and temporal local boundaries.

Along with Minkowski and Einstein in physics and mathematics, in psychology we know that we must not speak of space and time, but rather of space-time, not two things or processes but two dimensions of a single reality.

Some of you will recall the important experiments of Harry Helson on the skin. On the volar surface of the forearm he touched three spots between the elbow and wrist, equally spaced about 30 mm. apart and at the rate of about one per second. He then asked his observers to draw spots on a paper indicating how the three felt. Then he stimulated spot 1 and a second later spot 2, and then 0.5 second later spot 3. When they drew what they felt here spot 3 was only about 15 mm. from spot 2 and 2 was the usual 30 mm. from 1. This, Helson called the Tau effect. It showed that if you change time you also change space. Other work has shown that the converse also is true, that we can alter time by changing space.

But our present task is to complete some things discussed in our last few papers of this series. Later we will consider some of the above facts more throughly.

Köhler and his colleagues have shown by a brilliant series of experiments over a couple of decades some of the ways in which the inputs from the sense organs can be transformed and distorted, and how these physical effects in the media, the neural and biochemical systems of the body, may be either transitory or quasi-permanent. My own notion is that some of them, set up in very early childhood, may and do last the rest of our lives, always with the possibility of course of some amount of redintegration and change with age and the growth of the behavioral environment.

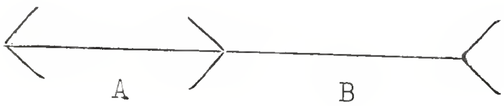
In 1947, Köhler and Dinnerstein published (Misc. Psychol., A. Michotte, Louvain, 196-220) a paper on figural after effects in kinaesthesis. Figural after effects in vision were known to be "exceedingly persistent." How long do kinaesthetic effects last? Let me quote only the conclusion found in this experiment. Tests after three and one half and five months showed that "the effects had just begun to decrease."

More recently Wertheimer and Leventhal at the University of Colorado repeated some experiments of the durability of kinaesthetic after effects (J. Exper. Psycholo., 1958, <u>55</u>, 255-257). They found that the amount of the residual FAE is essentially a direct function of the amount of inspection, the length of inspection time varied from 1 to 5 minutes. But more importantly they verified and extended the Köhler-Dinnerstein findings. After 8 weeks, 7 of their 9 observers showed retention of the FAE. After 16 weeks, 6 of the observers who were available showed the effect. After 24 weeks, 4 of 5 showed the effect, and after 32 weeks, only 2 of the available subjects were on campus. Both showed retention of the effect.

I can testify that when I looked first at Köhler's demonstration at our Vision Research Conference here and then again eleven weeks later, I saw the reduction in size. Paleness of lines and recession in space of the left of two test rectan-

gles. This was a dozen years ago. Soon I must run another check test.

One further experiment, rather two. One by Professor C. H. Judd many years ago. Judd showed that by training he could abolish the distortion effect in the well-known Müller-Lyer illusion.



Look at the above figure. How does the length of the line B compare to that of line A? B is almost always reported longer. If we ask you to move the middle wing to the right until they are equal and inform you of the size of your error, working thus "with knowledge" Judd found that it was a matter of simple learning to reach a point of abolishing the error or 'illusion.'

Köhler and Wallach showed that this effect can also be abolished (that is the error of judgment) by the figural after effect procedure. Since the time required for doing this is much less than the conventional learning time, this finding should give those interested in visual training procedures, their development and applications, much food for thought.

For those who desire a far more complete and careful study of these problems I can only refer you to the Kohle and Wallach monograph in Proc. Amer. Philos. Soc., 1944, 88, 269-357, and for the most recent expression of Köhler's ten years of work in neurophysiology underlying the satiation and figural after effects phenomena, to his paper in the Amer. Psychologist, 1958, 13, 150-155.

Are there other psychological phenomena dealing with the effects in time of present experiences on subsequent space-time, form, position, motion, etc., judgments? Yes, a number of them. And surprisingly some have never received the attention in subsequent research and thinking they manifestly deserve.

Purkinje, the Bohemian physiologist and experimental psychologist, made the pioneer observations on the stages through which the after image passes following stimulation. The fifth of these stages was a positive one. In 1916, P.F. Swindle became interested in studying this stage (Amer. J. Psychol., 1916, 27, 324 ff.). Swindle showed that if one restimulates at a point in time when the process is in its anaphase or increasing stage and after another properly timed pause restimulates again and again, then after an appropriate number of such restimulations the effect summates. You can go out into the daylight for an hour or more and upon returning to the dark room and extinguishing the light, you can project a bright, clear positive image upon the wall, floor or ceiling. Unfortunately we do not know how long the effect remains or what happens if we repeat the process enough to bring it to the maximum stability. We have repeated the Swindle experiment a number of times in our laboratory. The result is striking. Once we used a plaster cast of a hand and wrist and the observer on retiring for the night, eight hours later on extinguishing the light saw the cast clearly and sharply on the ceiling.

Professor Carr at Chicago, onetime told me he punched a hole in a sheet of black cardboard through which a slowly winking eye was the fixation target. The subject, hours later on retiring, reported that she was troubled by the eye winking at her from the ceiling of her bedroom. Motion must be written into the equation.

We have been able to show that micropsia, as shown by size-constancy measurements, can be reduced to the vanishing point by cheiroscopic tracing, far point tach, fusion alignment, etc., in about six weeks. The high percentage of the subjects showed no regression a year later. At present we are exploring the interesting possibilities of what we can do with three dimensional after image effects. To say that I am gratified by the results thus far would be putting it mildly.

The recent revival of interest in what can be done with hypnosis, and on the effects on certain of the so-called wonder drugs, on the functions of the backstroke

in instances of form blindness, etc., may indicate that a breakthrough is in the offing.

We need to know much more than we do now about such questions as just what do spheres, cylinders and prisms do anyway? What can one do by training and retraining, and what are the best devices and methods for accomplishing the desired ends? What is the relation between the two things, if there are two things, lenses and training?

Who can deny that some amount of acuity, astigmia (Gibson tilted line effect?), heterophorias, stereopsis, apparent size and distance, motion, brightness and color are functional and phenomenal effects? It is tragic but true that many member of both eye professions do. So far as I have been able to observe there is little or no reason, considering their training and indoctrination, why one should expect them to do so. But sooner or later there will come a day, science is moving on and up. Nothing has ever been able to stop it.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncon, Okla.

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshaw, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

March - 1959 Series 19 No. 6

To anyone who studies vision seriously and thoughtfully it is more and more apparent that the rigorous investigation of any visual problem calls for the talents and insight of people with advanced training in several divisions of science.

In the early history of visual science the 'natural philosopher,' the physicist, the geometer and the physiologist were those who brought the problems out of speculation and mere logic into the laboratories. With this came the inevitable broadening of the horizon. So it isn't at all strange that today, for example, research on the problems of myopia, brings in the biochemist, the experimental psychologist, the electronics engineer, etc.

Much of the pioneer work of course was done by the early physiologists. One of these in particular is the subject of this paper.

On August 31, 1821, a boy was born at Potsdam, near Berlin, Germany. This young man became one of the greatest of all the contributors to visual science. His name was Hermann Ludwig Ferdinand von Helmholtz.

In 1849 at the age of 28 he became professor of physiology at Königsberg. Very few attain professorial status at age 28. In 1885 he held the same chair at the University at Bonn, and three years later at Heidelberg. In 1871 he accepted the professorship of physics at the University of Berlin. His talents and prodigious genius were thus not limited to one single field. He dies on September 8, 1894. Recall if you will that the neurone was only discovered two years before his death and that the exact nature of nerve currents was not discovered until Lucas and Adrian did it in the years 1912-1914. These facts give us all the more appreciation of the genius of this remarkable man.

To say that Helmholtz was one of the very greatest of the contributors and advancers of physiological and psychological optics would cnly be stating the fact. He was also great in physics, particularly in mechanics, where he is regarded as one of the founders of the law of the conservation of energy.

In 1851 he invented the ophthalmoscope and retinoscope and the ophthalmometer, by means of which he measured the curvature of the lens for near and far vision, proposed an explanation of how accommodation takes place, showed how the two eyes move in synchronism to give binocular single vision, and along with Thomas Young, proposed the well known theory of color vision, which held that by mixtures of red, green and violet any visible hue could be produced.

Of equal importance was the publication in 1862 of his monumental treatise on Sensations of Tone. In it he gave the first accurate account of the mechanism of the operation of the three little bones of the middle ear, and proposed his well known theory of the resonance of the fibers to explain the action of the organs of Corti in our perceptions of the combinations of harmonics which give tone timbre.

Helmholtz was also famous for his studies on reaction time as a possible means of measuring the speed of nerve conduction. Gardner Murphy in his history states that physiology "changed from its abject position under the philosophy of nature to its independent magnificence at the hands of Helmholtz."

For a time Wilhelm Wundt was a laboratory assistant to Helmholtz and Wundt later founded, at Leipzig, the first laboratory of experimental psychology.

Another of Helmholtz's students, Hertz, while studying a problem suggested to him

by Helmholtz discovered the existence of electromagnetic waves, and this was the beginning of the notion of the electromagnetic nature of light. Hertz barely missed the discovery, a few years later, by Röntgen, of x-rays. The foundation has been laid by Helmholtz and Hertz.

The monumental work of Helmholtz on optics was, of course, his three volume treatise on Physiological Optics. The first volume appeared in 1856, the second in 1860, and the final volume in 1866, ten years after the first. They were in German and so the number of American and British students who could read them was limited. In 1921, the Optical Society of America met in Rochester, N.Y., and celebrated the centennial of Helmholtz's birth. Sponsored largely by Adolph Lomb, a dozen scholars set about the task of making a good English translation. Three years later, under the guiding editorship of Professor J.C.P. Southall, the first volume appeared in English, followed by volumes 2 and 3 in 1925.

Volume 1 contains an extensive anatomical description of the eye, after which some 200 pages are devoted to the optics, blur circles, the mechanism of accommodation, chromatic aberrations, entoptic phenomena and a full discussion of the ophthalmoscope and its uses. His discussion of the manner of image formation is the classical one and is still found practically unchanged in many books on geometric and physiological optics. But in the appendix of volume 1, (pp. 261-300) you will find a translation of the famous paper, written in 1901, by A. Gullstrand on image formation. In it he pointed out that Sturm's conoid of the ray sheaf "is a mathematical impossibility" and that the Helmholtz treatment of image formation has to be radically revised or abandoned. The Gullstrand appendix article is not easy reading, even if you are far better trained in mathematics than most of us.

Volume 2 deals with light and color, photometry, sensitivity and contrast phenomena. It is relatively unimportant to us today that there are serious objections to the Young-Helmholtz theory of color vision, and also to his resonance theory of hearing. Just recall the status of things about the time of our civil war in science and so our admiration and debt to Helmholtz will be with us as long as our civilization lasts.

Volume 3 deals with the perceptions of vision and with the problems raised by the visual illusions. A chapter on eye movements was no doubt the inspiration for the studies, later by Raymond Dodge and Erdemann, and others, on eye movements and the first scientific studies of reading. Visual space phenomena and depth perception receive more than 100 pages and the section on binocular, single and double vision almost as much. The 35 pages devoted to rivalry remained only a laboratory curiosity until Margaret Washburn showed it to be an absolute essential for stereopsis, and Baldwin and others linked these phenomena up with motion as the matrix from which space and form are generated.

The three volumes contain so much of both historic and modern import that I should like to cite one example of the keen and penetrating observational insight of the great Helmholtz. To quote, "If I shut my eyes and hold up me forefinger, and try to focus it without opening my eyes, the moment I do open them I see double images of it, indicating that the lines of fixation are parallel or nearly so, and therefore pass by the finger about equally far from it on both sides. But in some strange fashion, with my eyes closed, I do contrive to get a clear image of the place where my finger is by touching the tip of it and rubbing it with the thumb of the same hand, then indeed, even with the eyelids closed, it is possible for me so to focus my eyes that the moment they are opened, the finger is seen singly. It is the same way too, when I touch and feel an external stationary object." And, "When we have compared the perceptions of touch and vision, and thus at length found out the direction in which the observed object has to be searched for, the final result of it will be the localization also of the optical images that have originated elsewhere and of subjective stimulations of the retina and the nervous mechanism of vision."

Here, clearly, is the recognition of the

principle of external reference, and of the dependence of visual space and localization on tactual, kinaesthetic and motor processes and of the unity of the senses, for example, long before von Hornbostel proposed this unity.

Helmholtz made his observations before 1866, almost a century ago. We can only speculate on how much farther along the road visual science would be today had such observations received the attention, thought, the rigorous research and investigation they so manifestly deserved.

We have, it is true, come a long way. But there lies ahead a much longer way. We should bow in reverent gratitude and thanks to the works and memory of Hermann Ludwig Ferdinand von Helmholtz.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

OPTOMETRIC EXTENSION PROGRAM
1928

PSYCHOLOGICAL OPTICS

From the laborotory of experimentol psychology of Somuel Renshow, Ph.D., ot Ohio Stote University, optometrists receive some of the most voluoble procedures for opplied optometry.

PSYCHOLOGICAL OPTICS

April - 1959

Series 19 No. 7

Helmholtz, in the third volume of his monumental work, devoted much space in print to the problem of space 'out there.' But this problem has been with us for a long time.

In 300 B.C. Euclid puzzled over the fact that the two eyes received different 'images.' Leonardo da Vinci, the artist, scientist, and philosopher, struggled with the problems of depth and distance.

And, in A.D. 150, Ptolemy brought out the astonomical puzzle as to the 'real' and apparent sizes of the moon, which engaged the thought and argument of Kepler, Descartes, Lambert, and Helmholtz, Berkeley and Kant. Finally, Professor Boring (1943) set up a test to measure the simple relation of the angle of elevation and the apparent size of the 'moon.'

All of which brings us to the problems of space, size, and distance, fundamental visual problems, which, according to Miss Vernon, have been a "battleground of theories and the despair of experimentalists for many a long year."

I am willing to acquiesce to the first part of her statement, but not to the last.

Helmholtz pointed out that monocular vision can give only direction. Along with many others it was assumed that fixation was an intentional and voluntary act of fine eye movements which caused binocular images in the two eyes to fall upon corresponding retinal points and so to be seen singly.

While it is true that gross hunting movements may be voluntary, it is also true that precise alignments depend chiefly on involuntary mechanisms.

If you hold one finger fairly close to the eyes and another at arm's length, either finger can be seen singly and fixated by shifts in the convergence. But if you now try to hold stable fixation upon some point in space <u>between</u> the two fingers - it is impossible.

So fixation, which was supposed to be the simple means of getting two differing images simultaneously on corresponding points of the retina in order to secure fusion, turns out to be very complicated. Is mere location or retinal position the decisive determinant of good third degree fusion? What about the Panum areas which operate like Weber circles on the skin?

Koffka and others have pointed out that the real problem is why equal processes correspond to each other and not that energy impacts fall upon similar locations of the two eyes.

The proposed answer is that the like processes which become related over disparate loci and hence fuse and take on an additional dimension are contour processes. These Köhler has shown are figure processes and are processes whose neurophysiological correlates in the brain are electotonic ones produced by direct currents rather than by the alternating currents of the ordinary nerve conduction.

Helmholtz one time made a clever observation that if in a stereoscope the one eye sees a black figure on a white ground, and the other eye sees a slightly disparate but white figure on a black ground, a perfectly satisfactory depth impression is obtained. The two adjacent contours alternate and set up stable figure processes. Motion generates space, form and tridimensionality.

If I show you a black square on a white ground to one eye and a white square on white but bounded by a thin black line to the other, you will see second degree fusion but the two fingers will exhibit the well-known phenomenon of luster. But if now I remove the contour lines of the white on white figure, the same distribution of light to the retinas is given, but now there is no luster. Thus, the evidence is at hand that purely geometric and physiological 'explanations' of the conventional sort break down when we look for the answers to the questions about our perceptions of space and form.

The great anatomical gulf between the two halves of the visual field at the mid vertical meridian does not appear in perception even though as Curtis (1940), Bonin, Garol and McCulloch (1942), and Le Gros Clark (1942) have shown there are no neural paths connecting the left and right areas, seventeen, through the corpus callosum; and that conduction between areas seventeen and eighteen is so diffused that is is impossible to see how any brain energy paradigm could be the answer to the problems of the third dimension. Of course it should be borne in mind that because of the partial decussation of the optic bundle, half of the left eye is really right and vice versa, but again this could not possibly be the answer to the problems of the three dimensional single vision of our everyday experience. Just try a geometric projection!

The geometry of binocular space led to Vieth-Müller circles and thence to a model of the horopter.

But in 1902, Hillebrand made his famous alley experiments. And these we repeated and extended in 1913 by Blumenfeld.

These experiments were made simply to test the assumptions that the former postulations which sought to explain our perceptions of depth and distance could be ordered under the principles of geometric and physiological optics.

The alley experiments were simple. In a long, dark room, for example, you set up parallel pairs of small dim lamps. You are seated looking at a slight declination angle at the numerous pairs. There is no visible surround. You are asked merely this: With all the others turned off, to ask an assistant to arrange the successive pairs, starting from nearest, and in another series, fartherest from you to set the lamps at such separations that they appear to you parallel or equally distant from each other and in lines parallel from front to back or back to front.

When this was done, under careful conditions of control, the lamps, when measured as to separation distances from the midline, formed an hyperbolic series of separations. That is to say they had to be much farther apart at distance than at near to appear to be parallel.

When we repeated these experiments in our laboratory with luminous painted lines glowing under ultra violet illumination we got the same kind of results.

So, Rudolf Luneberg, an expert mathematician, came along. He saw that the alley experiments contradicted the simpler geometric assumptions of the Vieth-Müller circles or the conventional horopter. He asked himself the natural question; what is the proper <u>metric</u> for binocular space? So, he set about to take series of measurements and to test them to see if they would fit the assumptions of Euclidean geometry - the geometry of lines, and of angles. The alley experiments said no. So Luneberg went to work.

There are several interesting and important geometries of space. In topological mathematics, for example, the very irregular shoreline of a lake may be a more perfect circle than one drawn by a compass.

In the spaces of Riemann and of Lobachevsky parallel lines do not meet at infinity but converge or diverge. Luneberg said to himself what <u>is</u> the proper geometry of binocular visual space? What kind of geometry best fits the perceptual observations of people viewing the alley experiments?

Well, in brief, Luneberg concluded that the theory which describes binocular vision based on the convergence of the optic axes in the absence of monocular and experimental factors best is a Riemannian space of constant negative curvature.

To make the alley lights look parallel you had to place them proportionately farther apart, the greater their distance from the eyes. Parallel lines in the space of Riemann's geometry diverge with distance. In the Euclidean geometry parallels are equidistant regardless of the observer distance.

In the Riemann space, two factors had to be determined experimentally; two personal constants which differed from person to person. One of these constants is σ (sigma), the degree or fineness of depth perception, smaller for those less sensitive to the differences between convergence angles. The other constant was K, the curvature of visual space, in other words a measure of how much the visual space of an observer varies from that which one might expect if space were Euclidean.

σ (sigma) thus is a measure of the parallax threshold and is, in Luneberg's explanation to me, "a constant relation of depth perception to size perception."

K is the horopter, and, again to quote the late eminent Dr. Luneberg, 'is measured by a pair of targets in a wide field stereo at such distance from the eyes that the observer reports that now they are in one plane.

If the value of K is greater than zero, the geometry of space is elliptical. If it is less than zero, the geometry is hyperbolic. This is Luneberg's contention; namely, that the proper metric of binocular space is hyperbolic.

But I have one quarrel with the Luneberg theory. It holds that the ratio of depth or distance to size remains unchanged, but the apparent distance does change with convergence. But more about this later.

Luneberg's theory holds that if we know the personal constants, σ and K, for an observer, the whole of the geometry of visual space for this observer is determined. If true, this is very important for us.

But more about this next time.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshow, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

May - 1959

Series 19 No. 8

The perception of three dimensions is one of the most interesting and important problems in visual science. In vision the form, localization, color and brightness of things are revealed to us with a clearness and sharpness unrivaled by the other sense modalities, even though the absolute thresholds of some (smell and taste) are lower.

The facts are, however, that the sizes, positions, distances, etc., of objects 'out there' can be described by purely physical measurements and these can be related to each other and can be expressed in mathematical form. In previous papers of this series, I have attempted to show that perceived space relations may differ markedly from those deduced from purely physical and logical considerations, and that what are often designated as 'illusions,' wrongly called false or erroneous perceptions, are not false or erroneous in any sense, but arise simply because of the use of a wrong frame of reference.

The railroad track which is seen to converge at the vanishing point or horizon line we know is physically parallel. But it is also perceptually parallel if we view it from a helicopter looking downward. Confusion arises only when we fail to understand the right relations which obtain between those processes which we class as physical and those we designate as perceptual. Ernst Mach concluded that when we know enough, all of our descriptive accounts of all processes, molecular and molar, will be in terms of an out and out physical monism. This is a limiting case, and I fear that at present we are a long way from it. But, nonetheless, we are on the way.

Emmert's law, for instance, states that the size of the retinal patches stimulated by an object will vary inversely as the square of the distance from the observer. Spotnitz proposed, in 1938, that the perceived size of an object, which he called the 'imaginary image,' varies with the cube root of the distance. According to Emmert, the area of an object three times as far away should excite a retinal patch one ninth as large. But von Kleindt showed that there is no fixed relation between the size of the retinal patch stimulated and the perceived size of the object. In our summer laboratory several optometrists have shown that the addition of plus and minus lenses up to blur limits do not change the matches of the apparent target sizes from those made without the lenses.

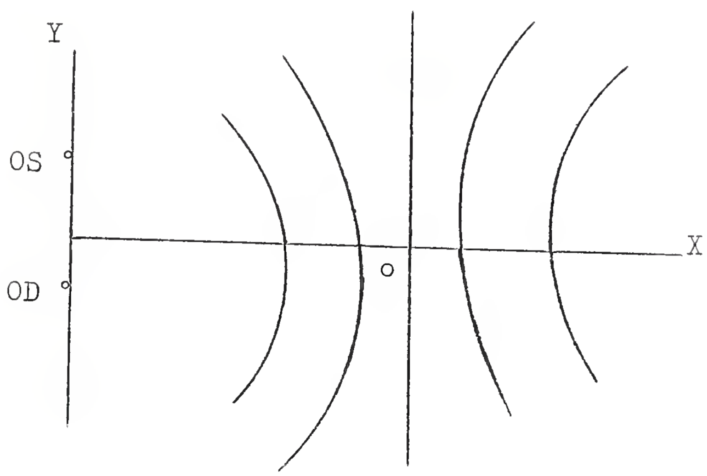
The results of numerous experiments in my laboratory previously reported in these papers (Vols. 10 & 11, 1949 & 1950) have conclusively demonstrated the facts of size constancy.

A square target at 6 meters is matched for size by one which is 529 sq. mm. in area. When we move the target twice as far away, to 12 meters, the match was 289 sq. mm. If the relation were purely geometric, in Emmert's sense, then at 12 meters it should be 1/4 of 529, or 132.5 sq. mm. Actually, it is 289, or more than twice as large as the purely physical conditions would predict.

This is the well known fact of size constancy; the tendency of objects in perception to maintain some approximation to the 'real' size under changed conditions of distance which physically should cause them to look smaller or larger, depending on image decentrations, observer position, training stage, lighting and contrast, etc., than they actually do. This Thouless called "phenomenal regression to the real object."

Helmholtz set up an experiment in which vertical white threads were arranged in a

horizontal plane to an apparently straight line perpendicular to the lines of sight. His results are represented in the diagram following:



At a certain distance X_o, characteristic for a particular observer, the arrangement is physically straight. But at nearer distances the threads are curved concavely toward the observer. At greater distances beyond X_o, convex.

These curves were called frontal plane horopters. They have been verified and measured by many investigators since Helmholtz. They prove that there are definite psychological laws of visual space and that these can be formulated in mathematical terms just as the physical relations can be so stated.

Each observer will have his own space lattice or framework, derived partly from purely physical or biological considerations and partly from the residual aftereffects of learning or training. Here vision adjusts to fit the concepts of space and form derived from movement, active touch and manipulation, kinaesthesis, etc.

The late Rudolf Luneberg pointed out that mathematically, the transformation of a melody by multiplying the pitches of the individual tones by a constant is motion; and that visual space has complete freedom of motion.

Here we must understand that a visual meter is only a meter if we specify where it is in relation to the observer. Space is perceptually anisotropic. A meter stick, or the Müller-Lyer figure, never looks the same in the vertical as in the horizontal orientation. Heinz Werner has shown that when a luminous vertical rod is seen in complete darkness, the alteration of muscle-tone by cold or by electrical stimulation on one side of the head neck and shoulders, produces the visual appearance of a tilt of the rod of from 8 to 12 degrees in the opposite direction from the vertical. 'Vertical' can thus only mean some dynamic relation of the spatial orientation of the observer and the seen object. No matter how precise the description of and measurement of the physical spatial object alone, the seen position is a transitive construction contributed by the observer. Why students of vision should persist in refusing to face such facts is probably due to their postulational commitment to physics and physiology as the all in all source of the answers. I for one, am convinced that this is wrong.

Luneberg saw this clearly. To quote him, "a situation of clueless vision is realized to a considerable degree in stereoscopic optical instruments, such as binocular microscopes, range fingers." And may I add x-rays, phoropters, etc.? "The purpose of these instruments is to determine the physical shape and localization of unknown objects by their apparent shape and localization in visual space. It is clear that their results can be trusted only if the relation of visual binocular perception to physical reality is known."

This is the problem of reductionism. Numerous experiments have been made, for example, on foveal brightness discrimination. An observer looks into a field restricting tube containing a spot of light subtending, say, two degrees. All of the periphery is black and formless. No matter what the results, all that one can say is that under the reduced conditions, seldom if ever seen in real life experience, brightness discrimination works so and so. How much can one safely extrapolate from such 'precision' experiments? This is the problem. How much can reduced measurements tell us about the vision of Joe Doaks, an individual, in the everyday operational demands of his occupational, recreational and personal living? How little or how much can one say about foveal

vision when the whole of the spatial summation from the periphery is nullified? The work of Granit and his students is a case in point. Our studies of peripheral stimulation in the improvement of foveal acuity can be mentioned as a case in point.

Visual space and form perception is one thing we must have in good operational order in order to live and to get along. Elsewhere I have shown that, when we compare congenitally blind children and adults with similar seeing children and adults, teleception (vision) plays a very different role after than it does before and up to about the age of puberty.

Our elaborate systems of communication, visual, oral and aural, and tactual-kinaesthetic, which are the very foundations of our cultural life are basic forms of space and form perception. We should study, develop and train them to the highest attainable orders of efficiency.

In actual practice we do not do this. Our present educational systems teach content largely, and implicitly hope that the growing child will by some fortuitous means develop, untutored, his ability to accurately perceive form, size, position, distance, color, brightness, etc.

Lenses alone cannot do this. Can phenomenal and perceptual functions be predicted by mathematical equations derived from purely physical and Euclidean geometric considerations? Some emphatically say yes. But if you re-read Vols. 10 and 11 of these papers you will conclude that there is another side to the story. There it is maintained that we must add additional parameters to the equations which are psychological ones. This is the issue.

In science truth is always bigger than little men. To know the truth is to make one free; free from unhealthy bias and limiting and deceptive concepts.

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshow, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

June - 1959

Series 19 No. 9

One of the very important developments in experimental and theoretical psychology, as well as in other divisions of science, came about during the years from about 1885 to about 1914.

This was the establishment of the facts of the unity of the senses. Mach was one of the first to show that much of what we see does not come from the eyes at all, but from the ears, the semi-circulars, the skin, the muscles, tendons and joints, and from the chemical controls of the endocrine system. No matter how carefully one measures any function of one of these, one must, if he wants precision, take stock and weigh-in the influences and contributions of the others.

Sherrington's famous Integrative Action of the Nervous System (1907) showed clearly that the brain, the central and autonomic nervous systems, have as their main function the correlation of the input impulses and the coordination of the components of the series of effector functions which comprise behavior.

This paper is intended to follow the general plan of this present series; that is to present some of the background developments which have contributed to our better understanding of, as the late Professor Kurt Koffka phrased it, "why things look as they do."

This paper has to do with the important contributions of a woman psychologist.

Margaret Floy Washburn was born in 1871. So she was about 14 years old when Wilhelm Wundt, once an assistant to Helmholtz, founded the first laboratory of experimental psychology at Leipzig, about 1885.

Miss Washburn was one of the early doctorates (in 1894 at age 23) from the then famous Cornell laboratory, under the guiding genius of E.B. Titchener. She spent a third of a century in teaching, research and writing, 29 of those years at Vassar. She became Emeritus in 1937 at age 66.

(One of my good friends, recently emeritus, was asked by a ten year old boy, "What is an emeritus professor?" His reply, E, in Latin, is a contraction of ex, which means without, and merit means merit!)

In 1927 the American Journal of Psychology published a Commemorative Volume, honoring Miss Washburn, containing scholarly papers contributed by 32 leading American psychologists; a volume of some 442 pages. The bibliography of her writings includes four books and 107 articles and papers, and 64 published reviews and notices.

Her first book, published in 1895 at the age of 24 was in German -- the English title of which was "On The Influence of Visual Associations Upon The Spatial Perceptions of the Skin."

In 1908 she published "The Animal Mind," a summary of the existing literature at the time on animal learning and behavior, and a classic in this field. Then in 1916 she published "Movement and Mental Imagery: Outlines of a Motor Theory of Consciousness."

In this book, Miss Washburn performed a great service; one that went a long way to show that perception, learning, judgment and reasoning were both cognitive and conative functions and that the essential role played by movements and movement mechanisms, so long practically disregarded by the associationists and structural psychologists, had to receive the emphasis these processes deserved in the functional and operational description and analysis of any and all human behavior.

In 1932, Miss Washburn was elected to membership in the National Academy of the Sciences. This honor was bestowed on her at a meeting in Washington where she presented her paper entitled "Retinal Rivalry as a Neglected Factor in Stereoscopic Vision."

This was followed by a paper in the American Journal of Psychology, 1934, 46, 632-633, on "Retinal Rivalry in Free Vision of a Solid Object."

In this paper, with P. Manning, she reported the fact that when one experimentally stops the natural and normal alternation of the two eyes, vision at once becomes planar and stereopsis disappears. Before her careful work and clear thinking, what was called "retinal rivalry" was only a laboratory curiosity. Following her lead, we have been able to show that it is not 'rivalry,' and that it is not 'retinal,' but that the things which alternate are figures. Things seen as ground do not 'rival' or alternate.

In the "Movement and Mental Imagery" the author states that "while the facts of attention, perception, and emotion have had their relation to body movement fully discussed, there still remain many phenomena connected with the complex life of the mind, the revival of past experiences and the construction of new thoughts and ideas, whose connection with the motor processes has not been satisfactorily traced."

Motor theory is discussed under two headings:

(1) the organization and grouping of movements into systems and sets during psychological functioning; and
(2) the relation of consciousness to movements.

The causal mechanism for the production of consciousness is the ratio of excitation to inhibition in the motor discharge. If the amount of excitation either sinks below a certain minimum or rises above a certain maximum, consciousness is produced. This second aspect of the motor theory, the excitation-inhibition ratio in the motor pathways as the agency producing awareness was frankly proposed as one contributing factor and not the sole cause of the appearance of perceptual or conscious qualities.

The first of the above two headings is the more important one, brought to the forefront by Miss Washburn. It deals with movements, their grouping, organization and variation in the organism, while it thinks, desires, searches, works, acquires knowledge, appreciates beauty or laughs at a jest.

Recall that, when she wrote, the basic problem of psychology had been to analyze experiences into what, then at that time, were the essential 'elements' of these experiences, i.e., sensations, simple images and simple feelings of pleasantness and unpleasantness. Washburn pointed in a different and radically opposed direction, namely, that the real problem was to describe the modes of performance of the total organism, part of which could be described in terms of the external stimulating environs and, in greater part, in terms of the trace systems left in the physical mechanism from past actions.

Thus, the hypothetical 'mind' or conscious life which was regarded then currently as almost wholly a matter of stimulus inputs and brain processes, in her clear and penetrating insight, became a matter of implicit and overt movement systems as the decisive and integrating agents of 'experience.'

She discusses the postural and phasic movement systems and the "tentative movements" which play very important roles in "silent thinking and reading." These she points out, evolved "most likely in animals with distance receptors," and thus delayed responses became possible.

Sets, attitudes, determining tendencies to specific forms of movements, and activity attitudes are discussed and the view is proposed that motor effects of stimuli "on the organism as a whole may be the basis of recall and mental imagery."

Judging and reasoning are processes in which tentative consummatory movements lead to the final solution. Thus, her emphasis was upon the integrated activities of the whole organism as a dynamic system, not as a mosaic of discrete and artificial part processes.

So, in the work of Margaret Washburn, motor theory was brought forth for closer examination and experiment at a time when all the emphasis was upon the input to the sense organs, and <u>before</u> the role of the feedbacks from the effectors had been demonstrated by Hudson Hoagland, Samuel Detweiler and others.

Her theory of "persisting tentative movements" was similar to Watt's (1905) use of the term <u>Aufgabe</u> for the "idea of a problem to be solved." She held that the problem-idea is merely 'persisting tentative movements' which continue from the time of stimulation through the response, or if they lapse momentarily, they renew themselves spontaneously (that is, through the mechanism of the feedback, but she could not have known this at this time).

She held that "all association is association between movements;" that the "essence of attention is movement," and that "all desires are ultimately connected with the great motor outlets of instincts."

It took a brave soul to break with tradition and stand up for the motor theory, as 'Maggie' Washburn did when she wrote Movement and Mental Imagery. She did just this. One thinks of Shakespeare's famous soliloquy: "What a piece of work is man, - In form and movement, how like a God."

In the modern day, Edwin Guthrie said in his book on Learning that "mental life is muscular life." Proof lies in Edmund Jacobson's Progressive Relaxation. And in the action of drugs which inhibit the feedbacks and produce form blindness; in polio, Parkinson's disease, etc. Without the action of the effectors there is only vergetative existence.

So, a 'bow from the hips' to Maggie Washburn, gone to her reward, for a life, devoted to science, which has materially helped to expand our horizons.

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshaw, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

July - 1959

Series 19 No. 10

In previous papers of this series we reviewed the history of the invention and development of the stereoscope by Wheatstone and Brewster in 1837. Because of their discoveries the visual perception of depth and solidity was brought into the laboratories for analysis and measurement. Among the various processes of binocular, stereoscopic vision studied there was one 'laboratory curiosity' called 'retinal rivalry' or alternation. Aside from noting the fact of the easily demonstrated existence of this phenomenon little or nothing was done beyond this.

Early investigators, for example, like Chauvaux and Trendlenberg, thought that rivalry or alternation was merely a hindrance to fusion. Miss Washburn of course showed that when alternation stops vision becomes planar. Thus, the fundamental importance of 'rivalry' had been completely disregarded.

Helmholtz, Volkmann, and Ruete proposed that it was due to shifts of attention. This was disproved in 1927 by J. P. Guilford. Very early Fechner, and in 1936 Brant Clark attributed the phenomenon to eye movements. In the same year Peckham showed that the eye movement theory was unacceptable.

Near the opening of the century B.B. Breese made extensive experiments on the problem. He studied the questions of forcing attention to change the phase; the effect of increase and decrease of light intensity; size of targets; and blurring of the targets. Unfortunately, Breese used only anaglyphs, black diagonal lines on red and blue-green backgrounds. With such targets there is a constant shift and it is difficult or impossible to maintain either the colored or black lines as figures. So about all that came from Breese was that the 'rivalry' function could not be studied by using anaglyphs.

In 1933 Miss Washburn proposed in her paper (Proc. Nat. Acad. Sci., 1933, 19, 773-777.) that "a neglected but important factor in stereoscopic vision is, not the combination of two slightly different images on corresponding points of the two retinas, but the alternation of these images in retinal rivalry." She showed for the first time that alternation occurs in our ordinary perception of solid objects, though we are unaware of it, and if alternation is stopped then there is no visual third dimension. To quote her further, "since solidity itself is primarily a motor experience, involving the movements of handling and grasping an object, it seems natural that it should be suggested, not by a static fusion of retinal images, but by an apparent movement."

So here was a hint that the stereoscope was also a stroboscope, although this was not made explicit. She said further that "rivalry occurs both in the stereoscopic slide and in ordinary vision, as one eye's image becomes dominant over that from the other eye, the apparent movement produced is identical with that which would be produced by part of the objects approaching or receding in the third dimension."

From physical and mathematical and psychological considerations depth could be postulated as a form of motion, static motion. Experimental psychologists are not troubled by the fact that we can perceive motion with nothing moving. Think of the well known phi effect of Wertheimer, which can be observed on the skin and in hearing as well as in vision. Then too, one may cite autokinesis. A point of light moves extensively even though the light and the body postures remain rigidly fixed.

In the phi movement, the red lights at the

grade crossing, a single light is seen moving back and forth. Neurophysiologists have shown that there is no difference in the distribution of currents in the brain between 'real' and apparent movement. I use two neon glow lamps to demonstrate the phi effect. If you maintain steady fixation for a period of about 20 seconds or more, suddenly the motion changes from left to right and back on the x-axis and changes to a smooth motion back and forth now on the z-axis.

The work of Professor Köhler has shown that currents in the brain produce anelectrotonic polarization effects in the cortical tissues at right angles to the retinal positions of the motion of the lights. So the change of position of the apparent motion fits in well with the concepts presented in previous papers of this series which reviewed and summarized many of the facts as to motor theory. The pace setting and syncretizing effects of the motor processes found in the feedbacks from the effectors are the perceptual factors of organization. The retinal processes trigger them off and this fact, demonstrated by Hudson Hoaglend and by Samuel Detwiler, is the basis for the postulation that in the final analysis seeing is essentially a motor process. Edmund Jacobson showed in his epochal studies on what he called progressive relaxation that when tonus is lowered below a certain level all visual and auditory imagery can be ablated or abolished even though the energy impacts, that is the stimuli to the sense organs, go on just the same. Further food for thought comes from the recent paper of Edwin H. Land, printed in the May, 1959 Scientific American and demonstrated before our Vision Research Conference here last June, that we can perceive full color in images which, according to classical theories, should be monochromatic.

So we live in a new day with a new set of concepts developing as to the matter of how we see things; how we perceive objects in space, time and motion.

In the early 1930's I started a series of experiments to find more satisfactory answers to a number of questions which were presented by the phenomenon then called 'retinal rivalry.' Under what conditions of test objects designs, sizes, positions, illumination levels, etc., does one get these interesting and important optical and psychological phenomena? How much does the rate of alternation depend on contours, contrast effects, on the relative widths of the black bars? Are there practice effects? Does the rate vary from near to far points? Are there age and sex differences? How does alternation rate correlate with parallax thresholds; with true stereo amplitudes? Etc.

Working with me on these problems were Hugh McFadden, Sybil Gramlich, and Edgar Chenoweth. Much credit is due them as well as a number of other of my students for the results of experiments which answer a number of the questions raised above.

In 1934 I made an experiment of myself and on another student to see what the effect of practice might be upon the rate of alternation. Split stereograms 87 mm. square contained alternate black and white lines each 3 mm. wide running to the upper left and right corners. These were viewed in a disparator and at the near point the subtense of the figures was 36°, 44'. In a dozen short practice periods my alternation rate started at 12 cycles or phases per minute and at the end had gone up to 26. Check tests were then made 3 months, 6 months and a year later and the rate remained at 26. In other words the function is trainable. I then made polygraph records on a number of subjects and measured the relative lengths of the right and left eye phases. There were no differences in the phase lengths for those who were right or left eye dominant and right or left handed.

I was able to show in another experiment that only things seen as figures alternate; things seen as grounds do not alternate. No differences in rate was found at far and near points, hence it was concluded that accommodation and convergence are not determining factors.

I then placed a Wratten neutral line filter before one eye reducing the light entering that eye by 90%. There was no change in the alternation rate, hence illuminance or brightness is not a determinant of the rate of alternation.

Then we made up a black and white positive transparency pair which we placed over Hering gray papers, Numbers 3, 7 and 28, thus giving a wide range of contrasts between the black diagonal lines and their contrasting ground. No differences in rate due to contrast effects were found. There were no differences between sexes, ages or subtend angles of the targets.

When we moved off axis into the periphery we got the following results:

Angle in degrees	0	6	8.5	11	14
Mean rate	32	19	16	14	11.6

Thus the rate slows down by about 1/3 when the target is moved off axis by about 14 degrees, and it should be noted that this is just about the limit of the normal form field.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1959
EP Foundation Inc.
(Nonprofit)
Duncan, Okla.

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshow, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

August - 1959

Series 19 No. 11

Last month I pointed out a number of important facts relating to figure alternation. The term 'retinal rivalry' was rejected because the function is not 'retinal' in the implied sense; and it is certainly not 'rivalry' in any meaningful sense, but rather a beautiful instance of complete cooperative effort in the production of the mechanisms which operatively produce our perceptions of the visual third dimension.

Washburn was the first to suggest that the key to the whole process lies not in the mythical concept of 'fusion' of static 'images' but that it is really a dynamic synergy of movements or motion processes.

I went a step farther and proposed that we can have static motions as well as dynamic motions. Such a simple contractile effect as picking up a pencil and holding it between the thumb and forefinger seems like a continuous contraction of the muscles. But is is not. The contractions are tetanus effects and are on and off about 12 times a second, which is the tremor rate. What appears to be a continuous process is therefore quite something else.

The sundry and various attempts to show that depth and distance perceptions arise from 'disparate images simultaneously falling on corresponding points' has been convincingly demonstrated to be untrue. Fritz Heider, for example, made an orthogonal drawing of a cube on a flat surface. This was viewed through a small monocular aperture. When a marble was rolled slowly across the flat figure, on reaching the drawing of the cube, defied gravity and rolled slowly up one side then across the top, then down the other side in the third dimension and then flattened out and rolled on across the table. Parallactic vision and perspective can be monocular as well as binocular to produce good depth effects. Ralph Evans, of Eastman Kodak, has made similar demonstrations at our annual vision research conferences, as a number of you will recall.

These things bring forcibly to mind the important fact that when any pattern of impression reaches a sense organ its eventual effect is some resultant of the stimulus and of the constraints or residual after effects, that is the frames of reference, which we lump together under the category of memory. In the cultural and ontological development, these latter processes may become more important in determining the final outcome than the input signals. After all, the input signals can only operate to trigger off some processes that are there and ready to go before the stimulus signal has put in appearance.

This fact is important, that the stream of behavior is not a mere series of end-to-end small part processes but that the essential characteristic is basically the whole act and that we get the components by an artificial and often spurious kind of analysis.

When we talk of figure alternation, and state that only figure processes alternate to produce tri-dimensionality what do we mean? What is a figure?

First, in dynamic or operational terms, it has been shown that figure processes differ from ground processes in several important respects. Figures behave differently in perception, retention and recall than do grounds. Figures form in the organic field by forces which are centripetal, that is they all tend to converge at a center; ground objects or regions have force systems which are centrifugal, that is they tend to move away and become as

disjoined from all other regions of the field as possible. Figure colors are surface colors; ground colors are film colors.

Figures form as the result of contours. Mach studied this problem for a number of years. Contours have to have certain physical properties, e.g., second differentials of brightness, in order that they may form the boundaries of a figure. Thus that 'envelope' or constraints of the figure are of the first order of importance, even before those things which comprise the figure are dynamically established. In previous papers I have shown that if the enclosing boundary of one series of lines in the left or right set of alternating line figures is removed, alternation stops at once.

These facts are convincing to anyone who takes time to study the problem that figures alternation measures a very basic and fundamental visual set of functions, by no means as simple as some would have us suppose.

I saw a 17 year old boy yesterday. The educators were stopped. Tests showed him highly intelligent; well motivated; a very bad speller, but they claimed a very good reader. My tests showed an alternation rate of 10. Auditory span of 5 digits. Tach span at 10 ms. 3 or 4 digits. Word 'blindness' and form 'blindness' pronounced. The right eye unable to cooperatively 'fuse.' Months of tutoring in spelling and reading left him with no gains, as you and I would predict. We must first build binocular vision, which is clearly his essential problem. An optometrist gave him glasses with plus a half on the right and plano on left with some astigmia correction on both, and wished him well. That these had no effect on his problem was immediately evident. No tests or measures were made of his basic visual functions, only "refraction." I think I can safely predict that three months from now George R. will be a different person, once we establish a new set of habits. It has been and can be done. It takes time, work and know how.

You may be interested in the results we have secured from some 200 subjects, using the 'standard' 3 1/2 inch square split targets with 3 mm. diagonal black lines at 45 degree angles, right and left. The figures in the following table are the number of right (or left) eye phases per minute. It makes no difference which you count, for we recorded the total duration of the R and L eye phases on polygraph tape for right and left handed and R and L eye dominant persons, and found no differences in the length or durations of the phases. The following table of percentile norms is based on 70 college students. The rate was counted for one minute, then a minute of rest was given and then a count was made for a second minute.

Figure Alternation Rate Norms

Percentile	Rate 1st min.	2nd min.
100	37	38
90	25	29
80	21	26
70	19	23
60	17	21
50	16	19
40	13	17
30	12	15
20	11	12
10	10	11
1	4	5

The mean, or average, first minute, was 15.67 phases per minute with its standard error at 0.778, and the standard error of the distribution was 6.47. The products-moments coefficient of correlation between the first and second minute counts was 0.91, which is very high. The low standard error of the mean indicates that the 'true' mean would probably differ from the obtained one by something less than 1 phase per minute.

Now finally a little bit about the interpretation of this table, for which I am indebted to the careful work of Miss Sybil Gramlich, in my laboratory.

A percentile table is like a hundred unit yardstick. Suppose a subject shows you 25 phases per minute. A look at the table tells that this is 90th percentile, so only 10 in every 100 people will do this well. 16 per minute is 50th percentile, which means that half will exceed and half will fall short of this. A score of 10 puts him in the poorest 10 percent of

the population in this respect.

Remember that these are statistical figures, that is measures of the distribution of the function in an unselected sample of the population. They are _not_ the optimal rates. Remember also that if the rate is 50 or more or only 6 or 7, the subject is very likely in trouble. We customarily train up the rate to about 24-28 cycles per minute and stop there.

When we do this we find other functions now operate better with nothing done directly about them. This, I feel, is a cardinally important fact about vision. In plain language it means that some of _this_ is also _that_.

But this is still another problem and I shall hope to be able to report more on it later.

VOLUME 31 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

PSYCHOLOGICAL OPTICS

From the laboratory of experimental psychology of Samuel Renshaw, Ph.D., at Ohio State University, optometrists receive some of the most valuable procedures for applied optometry.

PSYCHOLOGICAL OPTICS

September - 1959 Series 19 No. 12

The main objective of this series of papers has been to supply some of the background facts gained from early observations and experiments which have been largely instrumental in setting for us the framework of fundamental theory and methodology for our present and future activities.

It is a well known fact, for example, that in the history of science the early stages of development have been marked by a rigid emphasis upon thing concepts. Things were said to exist in their own rights and are possessed of certain attributes which define them both qualitatively and quantitatively. When these designated intrinsic properties failed to yield satisfactory accounts of form, size, position, motion, depth and distance, etc., accessory "cues" were invented to bear the burden of meaning. Only recently, as an example, I listened to a learned exposition of how the retinas and brain work to enable us to see things in the visual third dimension. The answer was a vague and undefined and nonlocalized hypothetical 'fusion reflex' which only succeeded in solving the problem by giving it another name.

As science matures 'thing' concepts gradually and often painfully have had to yield to <u>relational</u> concepts. In this type of thinking there are no permanent or fixed objects, but only ways of knowing <u>objectively</u>, and these ways are only relatively stable and are subject to change as the continuous reorganization of the relations of the physical and phenomenal worlds goes on.

The molar processes of perception and action and the organizing and integrating agency of the feedbacks from the effectors can be regarded as part and parcel of the molecular, biochemical and electrical processes of the body structures in isomorphic extension. Life and death in plant and animal cells, studied in the early years of the present century developed the physiological concept of the steady state. The processes of irritability, contractility, etc., were shown to be determined by such things as temperature, pressure, calcium-salt balance, etc. The resting state was never one of rest, but only one of relative activity of one kind rather than another. Stimulation upset the state of equilibrium and motor processes at once set about the business of re-establishing an harmonious state of energy conservation and expenditure between the living cells and their surrounds. When the energy exchange in the field reached the point where motion stopped, this was death.

It is important for us to remember that just as the organismic hypothesis received great extension and emphasis in the first two decades of this century, a similar kind of extension was taking place concurrently in psychology. And this it must be remembered was taking place at the time when behaviorism, conditioning and stimulu-response mechanism was the then current fashion. For instance, in 1921, Wilhelm Fuchs published a series of papers in Germany on the determinants of visual forms in perception. He held that forms became what they were in perception not because the things seen had intrinsic attributes but because the stresses set up in the binocular field gave rise to what Rubin, Köhler, Wertheimer and others were showing that form, position, distance, etc., were matters deriving from the relations with the field of living forces in the whole organism, not just on the retinas, nor not just in one part of the brain. Volumes 4 and 9 of these papers present much additional evidence. Nine was printed in

this series just 10 years ago.

When more than one region of the visual field is occupied by some form, it has been shown many times that even with two spots of light in an otherwise homogeneous field the apparent distance apart of the two as well as the locus of either can be changed materially by altering the relative brightness, or areas of the spots. In the year 1900, F. Schumann observed that the influence of motor processes on the sensory was marked. He hooded one eye and in a room with very low illumination showed a single point of light. He then instructed the subjects to look for the appearance of a second point of light a few degrees eccentric to the first. He thus could produce a monocular diplopia, since called the Schumann effect, by the active attention of the observer, a manifest form of implicit movement. D. M. Purdy saw the same effect later and stated that "the impressions of the fovea appear at the place where attention is directed, and all other impressions in the field change their apparent localization correspondingly." Bender and Teuber noted in the case of brain injured Navy men that the visual differentiation and recognition of a triangle as differing from a square could only be accomplished if they were permitted to trace the figure in the air with a finger.

One thing should be emphasized in any attempt to look at the background events and discoveries of science: Almost every important development has been a contribution of a number of people, and often as we have tried to point out in the matter of stereo and visual space, it has taken decades and even centuries. Popular biography has sometimes left the impression that the great contributions were made by sudden inspirations of revelations to one person. But this is rarely the case. Young students are often told of Archimedes' sudden solution in his bathtub of the problem of weighing an elephant; Newton's falling apple; Descartes geometrical discoveries in his bed; Darwin's evolution idea which came while reading a passage from Malthus; Einstein's sudden insight, while a clerk in the Swiss patent office, into the Michelson-Morley results and so on to the general principle of relativity; of the brilliant discoveries of Hertz, Steinmetz and scores of others one might name.

In no single instance was any one of these a sudden bolt from the blue. They were the final synergic seeing things together, based on large numbers of memories and the eventual integration into general principles.

The late Hans Zinsser about twenty years ago put it this way: "The scientist takes off from the manifold observations of predecessors, and shows his intelligence, if any, by his ability to discriminate between the important and the negligable, by selecting here and there the significant stepping-stones that will lead across the difficulties to new understanding. The one who places the last stone and steps across to the terra firma of accomplishing discovery gets all the credit. Only the initiated know and honor those whose patient integrity and devotion to exact observation have made the last step possible."

And, may I add that often there is the fact of serendipity, being at the right place, right time, right co-workers, right 'atmosphere,' etc.

In this series we have presented some of those who have opened up new avenues of approach to truth. It is proposed to continue and extend the list in the next series, and thus to do honor to those whose efforts have brought us a little nearer to some decent understanding of how we see things and relations.

VOLUME 32 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

OPTOMETRIC EXTENSION PROGRAM 1928 OEP

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLCGY

October - 1959

Series 20 No. 1

The basic need for a historical sophistication or perspective in any field had been pointed out again and again by many scholars. It makes no difference what division of science engages ones interests and efforts. Unless one knows the root origins of the theories, methods and crucial experimental results the present is seen in distorted perspective.

Such a limited person, to quote Professor Boring, "mistakes old facts and old views for new, and he remains unable to evaluate the significance of new movements and methods."

The whole history of science is a history of the change from the qualitative and uncontrolled observations, even of otherwise highly competent and clever individuals, to the rigorously controlled, measured and interpreted observations of trained perceivers in his field.

It is no discredit to point out this fact, for we owe much, very much indeed, to the scores of men and women whose pioneer observations - Galileo, Kepler, Newton, Berkeley, Haller, Müller, Goethe, Leonardo da Vinci, etc. - led to highly important subsequent developments in science. Science really came into being in a world where sharp issues had been raised by distinguished philosophers. In fact science, in its early beginnings, was natural philosophy; it dealt with things and processes in nature.

If one reads, for example, Whewell's History of the Inductive Sciences, or tries to follow the modern developments in the field of atomic physics or physical chemistry or modern mathematics, one is speedily convinced that no matter how competent and productive the researcher he cannot escape the demand of what William James onetime called 'thinking things together.'

The early beginning of biology as a division of science saw many important developments in the century from 1800 to 1900. The atmosphere was full of pro and con arguments about the mind-body problem; nativism versus empiricism; monism versus pluralism, etc.

The essence of scientific activity is that to be a scientist means first the setting up of a set of postulates, which must be rigidly adhered to. For example, if one starts with the postulate that the world of things or objects, stars, trees, dogs, flowers, people, exist; then the troublesome problem of epistemology (how one can know these things) is solved by leaving it to the philosopher, metaphysician and logician.

Another postulate says that these things exist, are real, in some amount or degree, and are therefore at least in principle, measurable in quantitative terms.

But to measure one must use a proper metric; some kind of quantitative scale; and one must justify both the scale and how it must be used in order to arrive at the truth.

This need for background; for knowing the historical trends and antecedents of modern activities is a very real thing. Recently, in a doctoral examination, a young man presented a very able series of experiments on the discrimination of brightness differences in the regions known as Mach rings or bands. A professor representing the graduate council, asked the young man the question whether his observers made subjective or objective measurements or judgments of the differences and their Weber ratios. I modestly restrained myself, for I wanted the worst old way to pin this good professor to the mat and force him to define,

rigorously and scientifically, the terms subjective, something not at all desirable but here and so must be tolerated, and objective, Ah! something real, precise, scientifically respectable. I am reasonably sure he could not do it. But this was neither the time nor place for such business.

In 1833 Johannes Müller became professor of physiology at the University of Berlin. He is referred to as the father of physiology because he emancipated it from the practical demands of medicine and made it a division of science in its own right.

Shortly before 1820, Ernst Heinrich Weber began to teach anatomy and physiology at the University of Leipzig. At this time and shortly after, almost all the experiments on sensory functions had been limited to vision and hearing.

Weber devoted himself to studies on the skin, on taste and smell, on the 'muscle' sense, and on pitch differences in hearing. Out of these studies psychophysics or the experimental psychology of intensity got its very important start.

Here again one sees in reviewing the history of science, how very often it is that important concepts develop from the work of several people, and often over more than a generation. Nature's most intelligent creature sometimes makes us wonder why it took him so long to catch on.

A generation before Weber made his measurements on the skin and kinaesthetics an able Frenchman, Bouguer, made a series of experiments on the sensitivity of the eye to light. Bouguer varied the relative positions of candles and pinholes to produce spots on a screen of varying brightnesses.

To make a weak shadow just perceptably different (either lighter or darker) from a shadow area adjacent to it, Bouguer found that the brightness of the two patches had to differ by a ratio of 1/64, or 0.01562. Even today our precise measurements of the DL's or difference thresholds for brightness do not differ materially from those found originally by Bouguer. But his work stopped right there and led to no general principle.

Then E.H. Weber came along. He measured the separation distance thresholds for two points on the skin; he studied temperature sensitivity; visual acuity, and in comparing the 'muscle-sense' in lifting weights with mere passive touch or pressure, he quantified the just-noticeable-differences, or difference limens or thresholds, and was the first to bring the results from all the sense modalities under a common or general or universal law.

Weber's law, stated in simple terms, is that the amount one must add or subtract from a given stimulus intensity to produce a just noticeable difference of intensity is a constant ratio: Delta I over I equals k. Otherwise stated the 'law' states that the magnitude of the relative difference threshold is independent of the absolute magnitude of the stimulus.

If, in lifting a 40 gram weight, you must add 2 grams or 5% to make the 42 gram weight just noticeably heavier in 50% of the judgments, then the same JND for an 80 gram standard weight will be 4 grams; and for a 160 gram standard weight it will be 8 grams, and so on. These experiments were published by Weber in the years 1829 to 1834.

Commenting on Weber's work, Professor Titchener in his masterful work on quantitative experimental psychology points out many of Weber's errors and points out that Weber's 'law' is a "very large generalization for so small a body of fact." But this does not detract from the historical importance of the work in impelling others to repeat and extend the observations, which has been amply done.

It is well known, for instance, that the 'law' does not hold for very weak or for very strong intensities, and so, as was once asserted, can not therefore be a special case of the law of inertia in physics.

And, as Dr. Florence Newman, in my laboratory, showed, in the field of taste sensitivity to the various sugars, it does not hold at any intensity level.

But, none the less, Weber, started something. He started experimental psychology on the road to becoming an exact science.

The astronomers classified the fixed stars by visible magnitudes and their eye-telescope intensity difference classification compares well with the best of modern photometric measurements.

The late Professor Titchener said that "if Weber laid the foundation stone of experimental psychology, Gustav Theodor Fechner (1801-1887) may be said to have planned, and in large measure to have erected, a whole building."

Fechner's work covered 50 years, starting with his quantitative studies of after-images in 1838 and terminating with the publication of the Psychische Massprincipien in 1887, which was just 5 years before the discovery of the neurone by W. His in Germany and Ramon y Cajal in Spain in 1892.

It was on October 22, 1850, Fechner tells us, that as he lay in bed awake and before getting up, the thought came to him of "making the relative increase of bodily energy the measure of the increase of the corresponding mental intensity."

Fechner was a dualist, as his Zend Avesta amply confirms, and his desire was to build a bridge across the gap between the physical and mental worlds.

More about this next time.

VOLUME 32 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright 1959 EP Foundation Inc. (Nonprofit) Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

November - 1959 Series 20 No. 2

Gustav Theodor Fechner was born in 1801. At age 16 he went to Leipzig University where he entered medical school, graduating with an M.D. degree five years later, in 1822. He remained at Leipzig the rest of his life, seventy years, and died in 1887.

The same year Fechner entered the University of Leipzig, E.H. Weber began his professorship of comparative anatomy there.

After Fechner had taken his medical degree, his interests shifted to physics and mathematics. He became professor of physics in 1834 at age 33. He published two papers on complementary colors in 1838, and in 1840 his famous paper on after-images. In 1839 he had injured his vision by looking through colored glasses at the sun. He had a 'nervous breakdown" as a result and for three years saw almost no one. But his vision improved and he could then return to active academic research and teaching.

Fechner knew about and was much influenced by the observations of Daniel Bernoulli (1783) who was one of the first to consider the problems of the relations of physical and discriminal intensities. He also knew of the pioneer experiments of P. Bouguer (1760) on judgments of the relative brightness of light spots. He also knew of the work of Steinheil who arranged the brightness of the stars in an arithmetic series and found that the photometric intensities to produce equal intervals of star brightnesses had to increase in a geometric series.

But the big influence was the work of his colleague, E.H. Weber, and his experiments on the skin, hearing, vision, etc., to discover how much added or subtracted stimulus intensity was necessary to produce the first perceptable judgment of greater or lesser. Weber, as we pointed out in the previous paper, generalized his observations in the proposed law that

$$\frac{\Delta I}{I} = k$$

That is that Δ I, or a small bit of I, added or subtracted to I, divided by I, is a constant. That is to say, the just noticeable difference in sensation is independent of the stimulus intensity but is a constant ratio. This was, and is, Weber's law. For pitch of a tone of 2000 cycles the value of <u>k</u> was found to be 0.003; for visual brightness k = 0.016; for lifted weights of 300 grams, 0.019, for tonal loudness at 1000 cycles per second at 100 decibels it was 0.088; and for pressure of 5 grams per square millimeter on the skin, the value was 0.136. So one could say that pitch difference sensitivity is about 45 times that of pressure sensitivity on the skin.

But right away certain fundamental questions arose. Is it legitimate to make such comparisons? Fechner was greatly impressed by such questions. His answer was, in general, yes. Sensations were mental quanta and could be treated mathematically just as could the physical stimulus intensities which produced them.

Fechner's great contribution was the publication in 1860 of the "Elemente der Psychophysik." In it he brought together by means of the calculus the differentiation and then integration of the results of many experiments by Weber and others, including himself, in a single general equation or 'law' for psychophysics. This he called Weber's law. Since then it is more commonly known as the Weber-Fechner law.

If <u>R</u> is the standard stimulus, and dR is

a very small increment of R, then the relative $\underline{R}$ increase is $\frac{dR}{R}$. Similarly if we let $\underline{S}$ be the 'sensation,' corresponding to $\underline{R}$ and dS, the S increment corresponding to dR. Fechner saw that he needed to develop a formula satisfying Weber's law (that dS remains constant so long as $\frac{dR}{R}$ is constant.) and the mathematical requirement that both dS and dR vary proportionately if they are quite small. So Fechner wrote $dS = C \frac{dR}{R}$

where C is a constant depending on the unit values selected for $\underline{S}$ and $\underline{R}$. This is Fechner's fundamental formula for mental measurement.

Without burdening the reader, this equation can be transformed into

$$S = C \cdot \log_e (R + C)$$

where C is the constant of integration. Since at threshold S becomes zero the formula then is

$$0 = C \log_e (r + C)$$

and so

$$C = -C \cdot \log_e \cdot r$$

From this

$$S = C (\log_e R - \log_e r)$$

and finally

$$S = k \log \frac{R}{r}$$

which is the form in which Fechner's 'law' is most frequently seen in textbooks; it says simply that sensation is proportional to the logarithm of the stimulus.

The constant k in this 'fundamental formula' is nothing but our old friend $\frac{\Delta I}{I}$ or the Weber constant. The law thus states that the 'sensation' will be equal to this constant (the least perceptible difference) multiplied by the logarithm of the stimulus. This is, you may note, a discriminal value multiplied by the logarithm of a pure physical stimulus intensity. Here the fun begins.

Scholars then, as now, lined up on the two sides of the argument. One group, including Fechner, said, "Sure its all right - go ahead." Another group said you can't add pigs and dogs, and also proceeded to challenge the 'law' on still other grounds.

After a quick look at some of these criti cisms it would be wise, if you are seriously interested in the fundamental theorems of measurement (and you should be) to go back to the 12 papers of this serie comprising Vol. 17 (1956). Read them car fully again. There we talked over some things about psychometric methods (which you use every day) and scales and scaling

Fechner was not too well regarded, we are told, by his contemporary physicists, and probably less by his contemporary philoso phers. But he did make a great scientifi contribution. He brought the problem of the precise measurement of human perceptual judgments back to the laboratories and out of arm-chair speculation and logic.

In a way Fechner was a rebel, because he dared to integrate, at a time when such was not "popular." He was in a way surprisingly inconsistent - for after his illness his writings were still strongly dualistic. Mach and Helmholtz found much fault, as did many others, with him. But he put the fat in the fire. If the 'fundamental formula' was wrong, said he, (an there was much evidence for it), go ahead and make a better one!

I can only cite a few of the criticisms here. If you are interested and have access to a good library, read the late P.W. Cobb's paper "Weber's Law and the Fechnerian Muddle" in Psychol. Rev., 1932, 39, 533-551.

In this paper Cobb, who had worked for years in the Nela Park laboratories on a variety of psychophysical problems in vision and has scrupulously examined all the published studies over the thirty years from 1900 to about 1930, came to the conclusion that Weber and Fechner had produced only a controversial and unsatis factory formula for the fundamental relation of stimulus intensity to perceptual intensity. The 'law' merely stated that if you have a measure of stimulus intensity (distal stimulus), if you take its

common logarithm and multiply this by the Weber constant, you can predict the 'intensity' or the magnitude of the 'sensation.'

The most competant experimental psychologists who had earned the right to an opinion by long and careful work disputed this formulation on several grounds. The S factor was a human judgment. As such it depended almost as much on the instruction, attitude, stage of training of the observer, and the method used for its determination, as well as the memory factors, etc., none of which were included as parameters in the 'fundamental' equation.

Fechner, for instance, held that S is a measurable magnitude or quantity, the sum of a certain number of sensation units. Titchener pointed out that "when a weak pressure is doubled, we have a more intensive pressure sensation - but this is a new S, not the old S with a certain plus added to it; the old S has entirely disappeared."

Fechner assumed that all JND's of S are equal, from whatever part of the intensive scale they may be taken. But we know that just noticeable does not necessarily mean equally noticeable, and equally noticeable does not necessarily mean equal. This is an important and far-reaching objection. JND's may be, and often are, relatively, not absolutely equal S differences.

In Vol. 17, it was shown that the several methods for threshold determination may give several different results. A threshold is a range having a lower and a higher limit and is never a single point. It has been shown in our laboratory by a number of doctorate research studies that in visual acuity (McFadden, Hudson) and on the skin (Rowland, Kraft) and in taste (Scofield, Newmann) the effect of practice and training is to narrow this range and to increase resolving power or sensitivity, that is to say that the k in the Weber law and in Fechner's formula is not determined by purely physiological or physical factors.

There are other still more potent objections to the classical psychophysical "laws," and there is a still more important development which had its beginnings even at Fechner's time. We will look at this next.

VOLUME 32 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

December - 1959

Series 20 No. 3

In 1801 in Brussels, Belgium, Joseph Antoine Ferdinand Plateau was born. Thirty four years later he became professor of physics at the University of Ghent. In 1843 he became permanently blind. He lived until 1883. So, much of his mature work came through the eyes of others.

Plateau's favorite subject of investigation was "subjective visual phenomena." In the eight years during which he had the gift of sight he accomplished much. We owe to him, first, the setting up of two disks with equal radial slits, which on opposite rotation gave the first stroboscopic method of studying the motions of a vibrating body, and which was the important forerunner of the motion picture.

We owe to him also the Plateau or geometric spiral which on rotation was the stimulus for inducing surprising effects, post rotationally, on fixated objects in the visual field. These were seen to advance or recede from the observer following stimulation. These effects were assumed to portray the effects of the function of accommodation. Clockwise stimulation with the Plateau spiral produced an apparent advance toward the observer of the fixated object. Counter-clockwise rotation produced recession of the object.

Some dozen years ago this phenomenon seemed to me as a possible means of training accommodation. With the capable aid of Hugh McFadden we set about to study these effects. We soon discovered that we could get either advances or recessions of any fixated object from <u>either</u> clockwise or counter-clockwise stimulation. This meant simply that we were dealing with another case like the windmills of Sinsteden. The whole matter was a dynamic relation set up between the observer and his egocentric locus and the stimulus. Sinsteden could reverse the direction of the windmill by merely imagining himself to be viewing it from the other side.

Perhaps the most important psychological contribution of Plateau came along about 1850. This was ten years before Fechner published his Elemente der Psychophysik. But Plateau did not publish until 1872, then in Comptes Rendues, 1872, <u>75</u>, 678 ff; and in the Bulletin of Belgian Royal Academy of the Sciences, 1872, <u>33</u>, 376-388. These papers were on the "law of the relation of the intensity of sensations to the intensity of the exciting cause (the stimulus)."

While he could still see, Plateau looked at various series of grays under widely differing levels of illumination. He reasoned that since the apparent relations of brightness among the different reflectances of the grays remained perceptually constant when the illumination was changed, nevertheless the <u>ratios</u> among the sensations produced by the grays remained constant or fixed.

Plateau thus concluded that Fechner's contention that the <u>differences</u> remain constant was in error and that the <u>ratios</u> were the true constants.

Fechner's solution to the problem was a logarithmic function, as we showed in our preceding paper. On the other hand, Plateau proposed that the true function was an exponential or power function. Many growth and decay processes in nature, even learning and forgetting data, are perhaps most aptly described by power functions of log epsilon, the base of the Naperian or natural system of logarithms.

Plateau expressed his conclusions in the form of a generalized equation:

$$S = c.R^k$$

Here c and k are constants, c relating to the particular modality such as seeing, hearing, pressure, etc., and k was the exponent of the amount of intensity change required to produce a just noticeably different 'sensation.'

This 'law' implies that

$$\frac{dS}{S} = k \frac{dR}{R}$$

and this was something quite different from the formula of Fechner. It makes the JND's relatively equal sensation magnitudes. That is to say the perceived size of the JND's instead of being a constant, as Fechner proposed, grows as an exponential function of the number of JND's above the liminal or just perceptable threshold. Resolving power of any sense organ, by such token, is always relative, never absolute.

So are contrast effects. So are size-distance judgments. In 1958, I published a paper in the J. Opt. Soc. Amer., 48, 790-793, on stereo decentration effects on apparent size judgments at various distances. The orthogonal equation we came up with stating the 'law' of crossed and uncrossed decentration effects on phenomenal size-distance judgments was a power or exponential function. With this equation we could predict the effect on size of decentration within 1/50 of a log unit, which is quite accurate.

This is cited merely as one, and in the past two decades there have been many more experiments serving to validate Plateau's original contention. His power function today seems much nearer the truth than does the Weber-Fechner log function.

You well may be asking, how do these functions differ? In an exponential or power function the values of the dependent variable (the 'effect,' S) will be in geometric progression if those of the independent variable (the 'cause' R) are in arithmetical progression.

But in the logarithmic function the converse is the case. The dependent variable is in arithmetical progression if those of the independent variable are in geometric progression. This is a radical difference as you may recall from simple mathematics.

Adding the logs of two numbers is the same as multiplying them; subtracting the logs is the same as dividing. This simple fact raises an ancient and honorable objection to the traditional Weber-Fechner psychophysics. William James stated it that no matter how many pinks you add you cannot thereby produce a red.

The power function proposes that equal sensation ratios are produced by equal stimulus ratios. This is perhaps as simple a statement of the modern version of the 'law' of psychophysics according to Plateau as can be made. For it we are indebted to Professor S.S. Stevens, who has also pointed out that some secondary departures from this law are bound to necessitate some amendment, as I have already pointed out with regard to field factors, memory traces, practice effects, etc.

The evidence from a large number of experiments all favor the Plateau principle. We may present here the values secured for the k exponents from some of these experiments.

Function	Exponent	Name of discriminal unit
Brightness	0.3 - 0.5	bril.
Visual distance	0.67	-
Visual length	1.1	mak.
Visual area	0.9 - 1.15	var.
Duration	1.05 - 1.2	-
Lightness, grays	1.2	-
Visual velocity	1.77	-
Visual flash rate	2.0	-
Taste	1.0	gust.
Heaviness	1.45	veg.
Loudness	0.3	sone

These exponents are the values of k in the Plateau equation cited above:

$S = c\ R^k$ in which S is the discriminal intensity or magnitude; R is the physical stimulus; c is a constant, and k is the exponent shown above.

So we seem nearer today than ever before to the time when we can precisely specify that when this amount of discrimination difference occurs, it follows from that amount of change in the stimulus intensity, or vice versa. If other field or

"noise" conditions obtain, then we have merely to modify the basic equation by additional additive or subtractive parameters.

If you put a pair of König bars in a 10 degree annular field to measure acuity and take in and out measures of resolving power, then repeat with a 20 degree field with the same bars, you will not get the same result. In size-constancy experiments if you reduce the angular size of the surrounding field from 48 degrees to 8 degrees with the test object remaining constant, we found that the judged size of the target was 53% smaller in the narrower field. So, one must conclude that within force fields the apparent shapes, distances, brightnesses, etc., are functions of the relational distribution of forces within the field. These too must be parameters in any final psychometric equation.

Measurement and counting are often confused. They are two very different things and demand quite different treatments and interpretations.

No professional effort of measurement or diagnosis is immune from the necessity of a clear and basic understanding of the fundamental theorems, facts and methods of measurement or counting and the interpretations which one can or must place upon them.

The real problem, as I see it, is psychologically this: What <u>are</u> the real determinants of any human judgment?

We owe much to Plateau. It took a century for us to catch up with him and so we pay homage to his clear thinking. The power principle is here now, and I feel sure, here to stay.

VOLUME 32 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1959
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

January - 1960

Series 20 No. 4

The general plan of this series of papers is to provide some information as to the background of important developments of the things which form the basis of present day theory and practice.

This time we go back to the year 1796. But before we begin the review of some basically important experiments let us digress for a moment.

In the course of his physical and social evolution, primitive man had to get food and shelter and keep his body warm. Some of our remote ancestors discovered better ways of attaining these ends. Flint arrow heads and knives did very well for a long time, until he found ways to dig ores out of the earth and to make iron, steel and copper. He invented the wheel, and later on he built railways and airplanes. He 'harnessed' the lightening; he developed language and communication; he wrote books; invented mathematics, astronomy, physics, chemistry, biology and psychology - the study of how he came to do these things which put him in command of his environs.

Our culture is a history of our actions. Thinking is a surrogate form of action. In the law, only your <u>acts</u>, can send you to jail. Seeing things, processes and relations is an intricate set of motions of total organisms. Perception is incomplete until it issues into some form of implicit or overt action and the feedback controls, organizes and reorganizes the time-envelope which brings the whole into spatial harmony and unity.

Alfred Binet, many years ago, pointed out that "the beginning of reasoning lies in perception." So the whole history of our culture begins with our better understanding of perception-action events and their determinants.

The scientific study of action had its earliest known beginning in the year 1796. Then the British astronomer-royal at the Greenwich observatory was one Rev. Mr. Nevil Maskelyne. In the observatory an astronomer watched the transit of a certain fixed star through the field of a telescope. When the observed position of the star fell exactly at the intersection of the cross-hairs in the telescope he had to note the position of a seconds pendulum, and thus set the master chronometer to the exact siderial time. This then was the standard for the chronometers of all British navigators. This had to be accurate.

In 1796 the man charged with this responsible duty was one D. Kinnebrook. Poor Kinnebrook was fired from his position because it was discovered that he reported the moment of coincidence too late. Other observers erred in the opposite direction.

These findings enforced the study of the <u>reaction-times</u> and the calibration of the observers. From it came the 'personal equation' and the whole system of studies of the normative type, that is the studies as to how and how much individuals differ from one another in many types of human functions. Thus statistics came into being.

One of the basic problems which grew out of the Kinnebrook case came to be called the <u>complication</u> experiment. In this an observer sits before a long pendulum which makes a single swing through an arc of 60 degrees. At some point, which is set by the experimenter, a click is heard in the headphones worn by the observer, whose duty it was to report the exact position of the pendulum in degrees at the instant of the click. Later we shall present some of the important findings

from these experiments. Here and now it it enough to point out that studies on space-time relations were among the earliest of psychological experiments. They comprise the first historical period in experimental and theoretical psychology. This has been called the 'astronomical' or 'personal equation' period.

The second period in our history came about 65 years later. Studies on the human voluntary act, or reaction, dates from about 1863. Prior to this time, Johannes Müller, often called the father of modern physiology, proposed that the fastest thing in nature was thought. Müller is quoted as saying that the speed of thought was something like six times the spped of light (which was then unknown!). Light has a velocity of some 186,307 miles per second.

The great Helmholtz, who lived from 1821 to 1894, and A. Hirsch (1863), Exner, (1873) and Dumreicher (1889) studied, experimented and published on this second period of interest and studies on human action time. This came to be known as the physiological period or the period in which the main emphasis was upon the efforts to measure the velocity of the nerve impulse. Keep in mind the fact that the neurone was not discovered until 1892 by W. His, in Germany, and by Ramon y Cajal in Spain, independently in the same year.

It was established that in unmyelinated nerves of vertebrates the nervous impulse travels at rates from 0.2 to 8 meters per second. In the myelinated sciatic nerve of the frog it varies from 24 to 38 meters per second and in human myelinated nerves it may be as rapid as 120 meters a second. This rate varies greatly with different animals, with different nerves in the same animal and in the same nerve under different physiological conditions.

Those who looked to physiology for the answer to the reaction-time problem met only with disappointment. If the time required to press a key after a flash of light was delivered to the eye was of the order of about 180 thousandths of a second and the distance from the eye to brain to code to finger muscles was say, 1 meter, then why should the reaction-time take about 18 times as the velocity of nerve conduction?

One thing became crystal clear! Reaction-times were something other than the velocity of nerve conduction. The pysiologists did not have the answer to the problem. So, this fact led to the third period in the history of human action.

This was the period of psychophysics. In previous papers in this series it was pointed out that during the years from about 1840 to about 1890 psychophysics was 'in the air.' This was the period of the intensive study of the relations of stimulus or excitor energy or intensity to the magnitudes of phenomenal discrimination or 'sensation.' From about 1865 the great F.C. Donders and his students studied the psychophysical aspects of "th duration of simple mental processes."

In 1889 Sir Francis Galton introduced reaction-time measurements as a phase of mental testing, one type of procedure being the so-called association-reaction procedure, used in lie detection, the diagnosis of certain types of mental disorders, etc. Here, not the meantimes, but the variances were the significant indicators sought. In 1890 Jastrow printed a selected bibliography of 57 titles of studies from many laboratories on the time relations of various mental phenomena. Choice type reaction times are used today as one of several means of determining the visibility of forms, etc.

The fourth period in the studies on action had been designated as the psychological period. It dates from the work of Oswald Külpe in 1893. The problem was the description and analysis of the 'typical action consciousness.' The significant statement, as I translate it from the Grundriss, says that "The reactions are nothing other than exact types or copies of what in the psychology of ordinary everyday life are called executant movements or manipulative acts (Handlungen). Hence the reactions maintain an extended meaning or significant beyond their finite durations."

Here was really an about-face. It said that actions do not begin with the stimulus and they do not end with the effector movements. Time integration could and did effect a spatial integration in the

learning and organization of movements. The perception of the consequences of the movements came very close to what is treated today as kinetic theory; these consequences could and did re-set the organism for subsequent acts in the series broadly called behavior.

Külpe's thinking clearly showed that the important psychological aspects of voluntary actions were not limited to the events occurring between stimulation and response.

Copyright
1960
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

February - 1960 Series 20 No. 5

Continuing the discussion of the study of our voluntary actions as psychological events, I should like to propose that we may add a fifth period to this important historical development. It is a natural and logical extension from the work describing the first four periods. It involves several important problems and developments.

The first of these may be designated as the study of the role of action in the development of our perceptions of space, form, motion, size, position, etc., of external objects and relations. Another is the extensive studies of the acquisition of skills, or voluntarily learned movements -- how the early <u>tension</u> movements, through proper practice and training can be and are transformed into an entirely different and much more efficient kind of movement system, called <u>ballistic</u> movements by Beaunis, Richer, Stetson, Coghill and numerous others.

Then too, there are the important developmental studies; such things as Heinz Werner's statement of the space and form perceptions of primitive races; of J.M. Baldwin's Mental Development in the Child and the Race; of the pioneering studies of Peiper, Minkowski, and of A.P. Weiss and his students at this university on the development of the various types of action from infancy to adolescence.

From Coghill, C.M. Child and his students and others came the description of the earliest movements of infants, called <u>mass-actions</u>, and the description of how individuation through differentiation and de-differentiation takes place. So the concept of the learning of a skilled movement as a mere summation or concatenation of hypothetical unit reflexes had to be abandoned. Hughlings Jackson, quoted by Sherrington in 1931, said that "The simplest spinal reflex 'thinks,' so to say, in movements, not in muscles." And in his 1943 revision of his Physiology of the Nervous System, J.F. Fulton said that "the central nervous system is organized, not in terms of anatomical segments, but in movement patterns." Recall the famous studies of the postural control of the body and its segments by Magnus and de Kleijn and the relations of these movements to the phasic or 'voluntary' ones, and it is at once evident that in the last three quarters of a century developments in experimental and theoretical psychology have had much to do indeed with the rather complete revision of the treatment of actions purely in terms of physiology and neurology.

In the first decade of this century the late Professor Titchener wrote that "the common denominator of meaning is kinaesthesis." But in any fraction the numerator is the term which defines the force, valence, the how much.

Meaning must be an active and transitive process of an ego, a self, a highly organized system of organic movements, the set, vector or direction of which is often largely determined by 'silent' processes, that is, processes which cannot be introspectively or consciously reported. These form the antecedents to action, the sets, purposes, intentions, motives, schemata, beliefs, etc., and which as development proceeds, become more and more time independent.

In the laboratory, suppose we wish to measure the time it takes to react to a stimulus and to find the answer to the question: Can one respond faster to a visual, an auditory or to a tactual stimulus?

The observer is seated before the appa-

ratus used in measuring and stimulus control, and the first phase or period of the procedure is called the foreperiod. In this the observer is given a careful instruction as to exactly what he is to do and how he must operate. This is the period of set, instruction. It is the period in which the type of reaction is set. There are three of these, called the sensory, the motor or muscular, and the natural or central.

In the sensory type the instruction would be something like this: Place the index finger of your right hand upon this key. Notice that about a meter away there is a dark screen in the center of which is a small round glass spot about the size of a dime. Back of this is a lamp, which will flash on at the same instant the chronoscope starts. You are to watch for the appearance of the light and press down your key as quickly as you can, which will stop the apparatus and extinguish the light. I will say "Ready" which means do everything getting ready to react which you have just been told; then about a second and a half later I will say "Now," which is a warning that you are fixated on the light spot and ready to respond, then after a certain time, the stimulus (light) will be presented and you make your response.

This sets the observer to make a sensory type of reaction; he is attending the appearance of the stimulus signal. The important thing about the period of instruction, or foreperiod is that the type of reaction is set; the implicit action is going on from the moment he hears the "ready." This is very important. It means much, for example if you know how to properly give a tachistoscopic exposure, and watching many work, I am convinced that many do not!

The second reaction type is the motor or muscular type. Here the instruction is different: "Place your index finger upon the key. Note the position of the light signal which will appear at some predetermined time after you hear the "Now." You are to keep your attention on the flexor movement of your finger. The visual signal is secondary." Understand?

The third reaction type is called the natural or central type. In it the instruction would run as follows:

"Briefly, here is a key and there a light will flash on. When the light flashes on you are to react as quickly as possible by pressing the key." The direction of attention and all the things which are controlled by instruction or set (that is the determining tendency) is some function of the observer; what he does and thinks is entirely up to him. He does 'what comes naturally' to him at the moment.

The second phase in the reaction experiment is, of course, the appearance of the stimulus, which touches off or triggers off the movement, set to go in a specific way by the instruction in the foreperiod. This is followed by key pressing or the response, and this is then followed by the final period, the period of the backstroke or feedback from the effectors, the perception of the stimulus and the movement and the set of psychological consequences of the reagent's perception of the series of events from the beginning of the experience.

Here are some average reaction times, in milliseconds.

	Reaction Type		
Stimulus	Sensory	Muscular	Central
Sight	270	180	190-220
Sound	230	120	140-190
Touch	210	110	120-180

It will be noted that the quickest reactions are to touch or contact stimulation using the motor or muscular type of instruction.

If two otherwise equal dash men are running a hundred yards in ten seconds, or about ten yards per second, and if one reacts to the starter's gun with the sensory and the other using the motor or muscular set or instruction, the difference can be as much as a tenth of a second and the motor person wins the race at the start by a yard or more, assuming that all other matters are equal.

When experiments are made instructing the observer to respond to the cessation of the stimulus rather than to the onset, that is to react when the light goes off,

one might logically expect that since the retinas are being stimulated and afferent impulses are already at the brain and are ready to produce faster movements that would be the case when light has to transform into nervous excitation. Professor Woodrow studied this question extensively. He found no differences in the speed of reaction between reacting to onset as compared to reacting to the cessation of the stimulus. If the nerve currents travel about 100 meters per second and the distance from the eye through the central distributing mechanisms to the muscles is about a meter, say, then the reaction time, in terms of sheer nerve conduction should be something like the order of ten milliseconds.

There are marked practice effects in reaction times, tending, of course, to shorten them.

A student in my laboratory gradually diminished the brightness of the stimulus patch from about 6 times the threshold intensity down to and even below the threshold. As the intensity decreased there was a regular and marked lengthening of the reaction time. When the light was made a little below the visual threshold, his subjects still reacted, the speed of the reactions lengthening to as much as three to six seconds. Here it seemed to us that the subjects must obviously be responding to the after-image rather than the sensory signal.

In a similar way if one lengthens the time between the "Now" and the appearance of the signal, Froeberg, Wells and others have studied this problem. They found that there is an optimum time interval which varies and is characteristic of the individual observer, and that change in either direction from the optimum increases the reaction time, the greater the distance the greater the deviation. This optimal duration was found to be different for the onset and for the cessation of the stimulus.

Let us look a bit further at another important aspect of time in our next paper.

VOLUME 32 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1960
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

March - 1960 Series 20 No. 6

Volumes have been written about time and the role it plays in human life. Plants and animals have periods of rest and activity, sometimes determined by sunlight, sometimes by other physical, physiological and psychological factors. Philosophers have long considered the problems of time and have come up with no less than three ways of looking at time.

Immanual Kant took the position that time or duration was a property like quality, given to objects by the observer in perceiving. This is the view of idealism which holds that the only 'reals' are mental processes.

Schumann and, later, Münsterberg gave time an exclusively sensory basis and proposed that this came solely from muscular tensions. This was essentially an isomorphic kind of physical monism.

Ernst Mach came to the conclusion that "time is the work of attention." Attention was defined as sensory clearness and this had to be some kind of sensory-motor action.

So, from the standpoint of philosophy and logic, the problem received no satisfactory solution. Up to about 1750 it was customary to regard the human spirit or soul as immortal and forever identical with itself. After this time more people began to believe that time was the essential factor in human life and that it modified all things, even the human soul.

Then about 1850 something else came on the scene. This was the doctrine of evolution as proposed by Darwin, Huxley, and Spencer. So the form of the physical body was, in this view, a product of time and change. Man evolved socially and culturally as well, and these upward developments were, in a very real sense, functions of time.

The late Professor L.L. Thurstone once pointed out that if time is an onflowing continuum, and it flows over one like a streaming fluid, then there can be for me only the present and I can know the past and future only as language constructions.

But, he said, if time is an all pervasive medium through which I am passing, then there is only the past; each succeeding second is past and I can perceive only the adjacent and immediate future, which is all there is before me.

Physical or astronomic time is a constant. The centimeter-gram-second system of measuring units give us the basis for communication and recording of when events happen, how much, etc. But in psychology things are not so simple. There are circumstances under which time drags or seems to stand still, such as waiting for a plane or train that is 3 hours late. At other times it races, as when you become completely absorbed in some interesting and important matter.

Certain drugs also induce these effects. Aldous Huxley made a notable contribution when in his little book, The Doors to Perception, he described his experiences after taking a physiological dose of mescaline, the peyote derivative. Atropine and scopolamine have very definite effects in that these specifically affect the feed backs from the effectors, and so change summation and space-time effects.

Time psychologically is very different from physical time. Through the functions of memory the past can be brought back (in most essential respects) into the present, and both past and present can be projected into the future.

Whereas the present for us in the sense of physical time is the infinitely thin line, the now, which separates the past from the future, the present psychologically is specious; it has extent, both spatial and temporal extent. Within the specious present all events have temporal parity; acts comprising a serial organization of movements become integrated and unified in a very special and important sense. Here one of the very highly important elements in the learning of any skilled movement is the co-action of expanded grouping and simultaneously the expansion of the specious present. These facts form the theoretical basis for certain types of visual training given to improved champion divers and swimmers.

Many of the very important developments in science originate from what seems to be very simple beginnings. Such was the complication experiment, which grew out of the early work of reaction-times.

Wundt in 1871 used two types of equipment, a seconds pendulum, the rate of swing which could be varied by changing the position of the bob; and a clock-like dial of 100 units which could run continuously at any set speed. He found that there are two types of errors, negative and positive; the first in which the judgment of coincidence (such as the hearing of a click in the headphones and the locating of the position of the pointer at that instant) and the second is the error in the opposite direction.

von Tichish studied other sense modalities and he confirmed Wundt's finding that the size of the errors and proportion of negative to positive errors change with the rate of oscillation of the pendulum or pointer; the errors progressively shift in the positive direction. C.D. Pflaum repeated and extended these experiments and confirmed the findings of Wundt and von Tischish.

Then J.R. Angell and A.H. Pierce at Harvard, using the same apparatus obtained contradictory results. They used three rates: 6, 3 and 1 second. They found a predominance of positive errors after practice and held that this was a practice effect rather than the clain of the Germans that speed was the only causative factor. The Germans denied practice effects.

Then M. Geiger came along and varied both speed and practice, working in both directions, and concluded that neither group was wholly right or wrong. Results were influenced both by speed and practice, and he added that 'manner of observing' was a factor influencing both magnitude and direction of errors. He classed his subjects as "reflecting" or scale division perceivers, and "naive" or index-hand perceivers. On the clock-type device he found more plus errors when the hand was moving downward and more minus errors when it was moving upward. Otto Klemm repeated these experiments and found that the 'zone of simultaneity' was quite variable and questioned the directional difference reported by Geiger. Haines' results agreed with Geiger. Up to this point what was accomplished was mainly pointing our problems and producing theories.

Then H.C. Stevens opened the next period. He set the observer to watch the moving pendulum, with the click secondary. Here he found that "the pointer carries the click out"; when coincidence was at 22 degrees the average perceived coincidence was at 30 degrees.

When the subjects were instructed to attend the click then the temporal displacement was negative, the average determination falling at about 12 degrees.

These results were summarized into what Titchener later called the 'law of prior entry': the stimulus to which we are predisposed by instruction has the advantage over its rival. It is perceived as occurring earlier in time.

Shaw and Wrinch, at the University of Toronto, made further advances on the problem. They showed that some intervals are judged more accurately than others; that this is contrary to what we would expect if the "Schumann-Münsterberg theory (muscle tension) was true. They found that discrimination is most accurate when we are least conscious of these feelings (tensions);

that when the strain of attention is strongest and surprise greatest, there the greatest errors in judgment are made. This 'feeling' (of stress) has its own temporal property, so the question naturally was asked: How can one time filling extent serve as a measure of another?

Craik showed much more recently that about 1/10 second divides visual stimuli in to slow and fast. If we take the unit range of the specious present, then we characteristically over-estimate lesser time intervals and under estimate greater ones. Time really is "of the essence" and like many other things, we need much more research on it to bring full and competent understanding, which today is fragmentary and incomplete, as is this brief account.

Copyright
1960
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

April - 1960 Series 20 No. 7

In the three preceding papers we reviewed the early history of the reaction and complication experiments - the first attempts to measure and describe the determinants of voluntary actions in space and time.

It was pointed out that careful experimental analysis showed that action begins well before the appearance of the stimulus signal, that is in the foreperiod of set and instruction. Certain effectors are increased in tonus and are set to go. Moreover sets of movements are conjoined in time, so that the reaction can be as simple as pressing a key or as complex as the concert pianist who plays from 'memory' or from the sight reading of the notes a score of perhaps thousands of sequential movements integrated by perception and learning into a single, unitary, harmonious and on-flowing whole.

The playing of the melody, theme, fugue, counterpoint, etc., cannot be described as a sequential series of small separate movements, linked together by any such process as association or conditioning. A note in the melody or a word in a sentence is not touched off by the one which immediately preceded it, but by all which have gone before and also by those which are yet to come. If you are puzzled by this statement, think of the mechanism of the delayed reaction, discussed in a previous paper of this series.

For the most part books on learning theory as a general rule neglect to give adequate consideration to perception and the part it plays in learning. This has no doubt been due to the hunt for some very simple mechanical explanation, such as association, conditioning, conduction chaining, cell assemblies, reverberation, etc.

Animals like rats, dogs, chimpanzees have good sense organs, brains and effectors and in their own environs cannot be as stupid or lacking in docility as we are often led to believe. They do learn mazes, Skinner boxes, etc., and Köhler's chimps came to discover how to secure food that was out of reach by joining together two hollow sticks to lengthen his arm. The animal 'catcheson' or gets insight suddenly, not by any summation of fractional parts of the final consummatory act.

Today we read a great deal about reinforcement as the mechanism of bringing about learning. This term means all those processes which follow upon the fact that when practice trials are successful and the animal is rewarded by food, this brings satisfaction and in some way increases habit strength and thereby the probability that subsequent practice trials will succeed with less time, energy, locomotion, etc.

Professor R.S. Woodworth has examined this problem (Amer. J. Psychol., 1947, 60, 119-124), in a paper entitled Reinforcement and Perception. In it he showed with clear and unimpeachable logic that reinforcement, whatever one really means by the term, must operate to change perception if any learning or conditioning is to be achieved.

When the question is raised: What is reinforced in conditioning? Woodworth answers that it cannot be the conditioned reflex because it has not appeared. Nor can it be the conditioned stimulus-response connection in the nerve-muscle pathways because they also are not yet formed. He proposes that it must be something in the organism which is 'reinforced' from the first trial and so must belong to the receptor-perceptual setting of the stage (such we may presume as the lowering of thresholds and the grouping of sub-regions

of the total field) rather than "the effector part of the organisms total behavior."

When, for example, in a multiple T maze a choice or discrimination point is reached, the decision to go left rather than right or straight ahead must be made in terms of the relative perceptions or meanings of the sensory stimulus objects as premonitory signs leading to the consummatory act, not to connections between motor responses which follow in time these processes.

It is this difference of sign quality of one path over the others which has to be 'reinforced,' that is, seen as an early stage preceding the right movement. This was the position of the late Professor E.C. Tolman.

This position is quite like the thing we do with language and logic when we 'reason' out the solution to a problem by weighing all the given facts and inferences and selecting the one which is the best fit.

It was Woodworth's contention that in order to influence the amount or rate of learning, reinforcement must be effective in perception. He said that "the conditioning experiment is really concerned with the establishment of a new perception."

Conditioning theorists have placed great stress on the reinforcement of the movements, such as bar pressing or string pulling, which gets the animal food. Woodworth does not deny this, but holds that the change in learning is really in the perception of sensory signals which conduce to the right rather than the wrong set of movements. I have pointed out that perception is a form of prodromal action which syncretically becomes an early stage in the consummatory act. This is the means of the expansion of the space-time envelope of behavior.

At about the same time Brogden and others showed that sensory signals may combine and interact without any reinforcement. Some twenty years before this Dr. Carl Rexroad in the Ohio State laboratory studied choice reactions in humans with a 5 finger reaction key to stimuli which consisted of several colored lights. If for example a blue light was shown and the right response was to depress the index finger, but if any other finger was used this was wrong. Right responses were rewarded and wrongs punished with mild shocks. Then he made experiments in which rights were punished and wrongs rewarded; and still others in which both rights and wrongs were rewarded and punished. He found no differences in learning in any of the conditions and concluded that where either reward or punishment operates as a reinforcer it 'works' because of its didactic or perceptual gain in significance.

Back in 1907 Sherrington pointed out the essential integrative function of the nervous system, and von Hornbostel emphasized the principle of the unity of the senses, all the inputs from which are correlated in the midbrain. But the Pavlov investigators raised another question involving the perception-learning relation. This was called stimulus generalization in conditioned responses.

Pavlov postulated cortical irradiation followed by concentration or pathway formation. He thought that irradiation accounted for stimulus generalization and concentration for discrimination. But experimental results and neurological facts did not agree. Lashley and his students, for instance, held that the gradient of habit strength is a product of variable stimulus thresholds rather than irradiation, and that so long as a single stimulus is used in conditioning generalization does not occur, but is some function of differential practice or training with two or more stimuli within the same dimensional continuum. This they held was a function of the total integrated organism and only secondarily a consequence of the physical properties of the stimulus.

So we may see that the trend in recent years has been to break down the barriers and show that perception, learning, habit and memory are all merely aspects of one organic behavioural reality, that is a very complex set of sensory-cerebral-motor-feedback processes, always set in the referential frames of set, purpose, attitude, motive, hope, fear, autisms and ego involvements and the welter of habit mechanisms already there and their compatability

to the present behavior demands.

I must dissent from Woodworth in one important respect. I cannot accept the dualism of sensory <u>and</u> motor processes. Without motor mechanisms, there is no sensory processes; and without sensory processes there are no motor processes.

All of the preceding arguments are merely to say that theories of perception and learning must be genetic and developmental. One learns a finger maze with the right hand. But the learning is such that he can perform the maze about equally well <u>with the left hand</u> or, as Miss Charlotte Rice showed in her experiments, with the stylus operated by the left or right foot. Learning is not a matter limited to specific muscle groups. This is particularly true when learning passes from the tension movement to the ballistic movement stage.

So the studies on action have given new emphasis to sense-perceptual and motor integrations, and by the same token pointed to the necessity of finding out more about how the infant puts on his amazingly intricate sets of perceptions and habits.

Next, let us look at some of the real fundamental studies on these problems.

VOLUME 32 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright 1960 OEP Foundation Inc. (Nonprofit) Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

May - 1960 Series 20 No. 8

Last time we looked briefly at some of the problems of the systematic relations of perception and learning. Both of these categories are man-made labels, pasted on selected instances of organic behavior in the most complicated and ever changing sets of both internal and external environs. Often the words used in such cases are reified, or made into things, and a specific interest or theoretical bias causes large and important segments of organismic determinants to fall within a large 'blind spot' and be disregarded.

The net results is that when we examine the experimental evidence and theoretical conclusions as to these processes we meet large amounts of confusion, bias, argumentation, and often dispargement and even occasional acrimonious name-calling. For example, in the January, 1960 number of the Psychological Bulletin, Donald J. Lewis wrote an impartial summary of the experimental and theoretical contributions to the problems of reinforcement, discussed briefly in our last paper. This review covered the work of the past ten years. In it Lewis discussed partial reinforcement. His conclusions are that there is no satisfactory explanation of the way in which reinforcement operates in perception and learning.

The data and conclusions from long lists of experiments are contradictory and conflicting, and are often marked by the absence of the right kinds of data, and that from these there are no parametric laws. Very few of the studies were interested in how one variable relates to others along the major ranges of both variables; and he showed that varieties of apparatus and methods made any generalization unsatisfactory. He concludes his summary with the statement that he "remains almost as empty of understanding of partial reinforcement now as when he began to review the literature, but he still considers the problems in the area to be fascinating."

The facts that total or partial reinforcement exists and that it is factor in perceiving and learning and problem solution cannot be denied. The questions are: How does it work? How much does it affect the amounts, rates and limits of the practice effects? How can we predict and control it? We must remember that from the Aristotelian account of nature in terms of his 'elements' - earth, air, fire and water - to the organic chemistry of today (and tomorrow) is a long way. The life sciences had had to wait for physics, chemistry and mathematics. So let us look at something else in the perspective of history.

In the year 1891 a young American student took his doctorate at a German University. His dissertation dealt with some important phases of after-image phenomena. Later he returned home and taught at Brown University. Here he was perhaps the very first to report the fact that after prolonged fixation upon a point in the middle of a curved line, a straight line through this point of fixation appeared curved in the opposite direction (Amer. J. Psychol., 1888-89, 2, 326-328). Little or no attention was paid to this fact. Some forty years later Professor J.J. Gibson noted and studied the effect, and proposed an 'explanation' that was normative and statistical in character. This stimulated Professor Wolfgang Köhler and his co-workers to dissent from Gibson's interpretation and to set about the making of extensive experiments to measure and describe the processes involved.

Köhler named the phenomena figural after effects and showed that seen figures not only behave differently in perception, re-

tention and recall from ground or surround figures, but also that, after extensive measurements with electroencephalographic apparatus the figural after effects must be produced by direct currents in the brain rather than by the alternating currents of the conventionally understood nerve conduction. This lasting anelectrotonic effect Köhler called satiation.

But just as important perhaps, there was something else. Professor Delabarre was an inspiring teacher. In one of his elementary psychology classes sat a young man who 'caught on fire.' His name was G. E. Coghill. Years later Coghill wrote that "this course left me with an inquiring state of mind concerning what seemed to me to be the fundamental principles of psychology--the nature and interrelations of sensation, perception and thought." So, Coghill decided to enter upon graduate work in psychology but came to realize that the approach "to the kind of psychological information I wanted lay through the physiology of the nervous system." This had to be approached through a thorough knowledge of the anatomy of the nervous system "about which I knew nothing."

At the University of Chicago later on he came under the inspirational teaching of the Herrick Brothers. There Coghill found that "certain fundamentals for physiology and pathology were there: the neurone, the conduction path, cortical localization of motor functions, sense organs, etc.; all leading to the conception of the nervous system as a telephone or telegraph with its conductors carrying 'messages,' a conception which seemed to me utterly inadequate and misleading." Thus Coghill wrote in 1929. He had become convinced that much more must be known about parallel studies of the development of behavior and of the nervous system.

This was essentially unexplored territory. But fortunately, at Chicago, upon the advice of the Herricks (who as undergraduates at Denison University in Ohio bought a cheap, second-hand printing press and with the late Professor F.L. Landacre, anatomist at Ohio State, they founded, wrote and printed the first volumes of the Journal of Comparative Neurology). Coghill started his work on the relational description of the development of behavior and of the nervous system in the lizard-like animal, Amblystoma.

It is important to remember that during the first two decades of the present century other highly important biological developments were being discovered at Chicago. I refer to the work of Professor C.M. Child and his studies from about 1909 -1912 and thereafter on the concepts of gradients and their formation, on the dominance and isolation of parts of the organism, etc. There also was Professor R.S. Lillie and his important contributions on protoplasmic action and nervous action; and later Professor Paul Weiss (1924) with many important studies on the embryology of form and on studies on regeneration.

There is some of the 'atmosphere' in which Coghill lived and worked, later at the Wistar Institute in Philadelphia and still later in Florida.

There were many highly important developments in biological and psychological thinking during the first third of the present century. For instance, Spemann in Germany and simultaneously Speidel at the University of Virginia won Nobel prizes by showing that when cells from one of the three embryo germ layers are surgically transplanted to another layer, cells which in situ would 'normally' have developed into an eye, an arm, or a liver, after transplant to another layer developed into something else which conformed to the normal organismic pattern. The 'fate' of what the cells ultimately became in development was thus demonstrated not to be self-contained within the cell from its beginning. And so our friends the geneticists had more work to do. And, the whole question of the relations of the nervous and effector systems and behavior in development took on new and highly important directions.

The foregoing now paves the way for a statement of some of the important contributions made by Coghill, Child, Weiss, and others which are very fundamental for us in coming to a better understanding of the organic operations which we classify as perceiving and learning. This we shall try to do next.

VOLUME 32 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1960
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

It is now approximately a third of a century since G.E. Coghill published the main results of his studies on the fundamental problems of the relations of the behavior of organisms to the anatomy, physiology and biochemistry of the nervous system. His contributions were of great biological and psychological importance. Like many other such things, it is only fair to say that a considerable number of students and researchers who deal with these problems have not carefully read and studied the import of the contributions of Coghill, Child, Weiss and others on these basic problems. The facts which they discovered led to an entirely new and different approach.

For those who wish to consult some of this important literature I would cite the following references:

Coghill, G.E., Anatomy and the Problem of Behavior, MacMillan, N.Y., 1929.

The Structural Basis of the Integration of Behavior, Proc. Nat. Acad. Sci., 1930, 16, 637-643.

The Genetic Interrelation of Instinctive Behavior and Reflexes, Psychol. Rev., 1930, 37, 264-266.

The Biologic Basis of Conflict in Behavior, Psychoanal. Rev., 1933, 20, 1-4.

Space-time as a Pattern of Psycho-Organismal Mentation, Amer. J. Psycho., 1938, 51, 759-763.

And, there is the excellent symposium edited by Allen D. Bass, The Evolution of Nervous Control from Primitive Organisms to Man. A.A.A.S. Symposium, 1959, Vol. 52.

Then there is C.M. Child, Patterns and Problems of Development, Chicago Univ. Press, 1941, and Paul Weiss, Genetic Neurology, 1951, Chicago Univ. Press.

The prevailing view of the relation of the development and functions of the nervous system to behavior, in its broadest sense, in the period from about 1920 to 1930, was based on the facts known at this time about the neurone, the sense organs, conduction pathways and the cortical localizations of motor functions. There were also the 'classical' concepts of the reflex and the conditioned responses of Pavlov, Bechterew and others.

From the body of such knowledge came the assumption, not based on complete facts, that the nervous system is a telegraph or telephone like mechanism in which the sense organs bring in "information," signals or "messages" to the brain, which in turn, codes and decodes these inputs and sends out appropriate messages to appropriate effector groups the orders to contract, secrete or relax in some complex spatial and temporal order, thus constituting the acts or the organism in its transactions with its environs.

It was this view which Coghill could not accept, claiming as he did that it was "utterly inadequate and misleading." Nor was he alone in his unwillingness to accept the view that the acts or organisms were mere passive concatenations of assumed elemental reflexes, determined by the attributive properties of the stimulus objects 'out there' and the resulting propagation of the weak energy impacts through conduction channels which were established in the sensory-cerebro-motor-feedback system by some hypothetical process of synaptic resistence reduction, reinforcement, conditioning or association.

Opposed to the over-simplicity of such

and analysis and synthesis of behavior in biological and psychological terms were a number of able scientists and philosophers of science, who like von Ehrenfels, Husserl, Meinong, Wertheimer, Köhler and Koffka looked at the organismal hypothesis, at the integrative nature of living organisms, and concluded that whole movements, perceptions, memories, problem solutions, are never mere summations of smaller, simpler part processes. A skilled movement could not be analyzed into a serial set of simple, independent 'reflexed' or muscle twitches.

Extensive experiments on visual apparent movement phenomena, form perception, size-distance relations, learning, memory and problem solution could not be satisfactorily 'explained' by the conventional and uncritical atomistic and elementraistic theory as to the true role of the relations of the neurological, physiological, biochemical and psychological functions in the organism as it deals in an integrative way with its internal and external environs.

The physical and biological facts about neurones, nerve conduction, etc., were not questioned. The objection was simply that the complex forms of the behavior of vertebrate organisms, basically integrative in character, could not be satisfactorily described, analyzed and accounted for by the assumptions of an out and out atomism. A melody, the objectors said, is musically something more than a mere succession of single tones. It has form, plot, design, schema. It has perceptual and emotional properties and is not the same from organism to organism. How we perceive and respond to it can not be decided by no matter how detailed a description of the pitch, intensity, tonality, vocality, or volume of its individual components. An essential part of the complex process is supplied by the organism. The end result is something more than can be predicted from no matter how much one may know about 'the stimulus.'

So we have two radically different and opposed ways of looking at the problems of behavior and their relations to the mechanisms of organisms. And it is only fair to say that in 1960 and extensive amount of research, thinking and practice is still based on the telegraph-telephone concept of how the nervous system works in integrating the serial acts of the organism. There is also a wide interest and activity on the other side of the fence.

I may take the liberty to quote from a recent letter from the director of research from a large optical company as illustrative of the change that is in the air. "Thus far, we have felt our mission to be primarily the production of a sharp image on the retina. Even with this objective, compromises must be made which require the weighing of the relative importance of viewing distance and peripheral versus foveal vision--compromises which are now based on rather incomplete understandings. But since the perceptual process involves factors quite beyond the optical image on the retina, an investigation of these factors should permit us to make improvements of a more fundamental nature than those we are making at present." This is the picture. Seeing is something more than spheres, cylinders and prisms. There is no question as to their importance. But there are other parameters in the equation of equal or even greater importance.

The facts are that today, more than ever before, industry, the military and private and professional research is coming to have to face up to the problems of the expanding demands of our culture upon the integrated organism. If you were in a fast airplane, at high altitude and made a certain dive and leveled off, you would soon be in a weightless environment; gravity at zero. Under these conditions, what happens to visual acuity, space and form discrimination, time estimation, etc.? If you are alone in a 'capsule' simulating a missile flight, what happens to your discriminal functions under conditions of isolation? Or, as Professor Werner showed, if you are looking at a luminous vertical rod in a completely dark room and through temperature or other change, the muscle tone of the muscles of your neck and back are altered on one side, then the vertical rod appears no longer vertical but slanted off about eight to twelve

degrees in the opposite direction. The vertical visually is thus a dynamic relation of the test object in part <u>and</u> of the dynamic status of symmetry of the posture regulating mechanisms of the body.

So the plot thickens. Let us look next at some of Coghill's important contributions to the background for our thinking.

Copyright
1960
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

July - 1960 VISUAL PSYCHOLCGY Series 20 No. 10

It was the problems which arose from considerations of the parallel development of behavior and the nervous system that occupied G. E. Coghill during most of his active research career. He studies these processes in Amblystoma, a lizard like animal.

Let us look at some of his findings. In the embryo from the first the principle of polarity obtains. This means an electro-chemical gradient is a primary characteristic of the cell. The basic and bedrock function of the living cell or organism is metabolism.

As the embryo grows, develops and differentiates to the point where muscles develop in the dominant or head end these primitive muscles can be excited to contact by local stimulation (the stab of a needle or an electric shock) but cannot be excited by a touch on the skin. Shortly after with increased growth a light touch on the skin elicits the early flexure stage, which is soon followed by the coil stage, that is the body of the animal bends into a tight coil. This is then followed by the S reaction in which there is the reversal of flexure before it completely becomes a coil. The S finally follows the development of the tail regions and the serial performance of the S reactions make swimming, and finally with the coordination of the rear legs, land locomotion and achievement. This, Coghill said, was one of the most significant landmarks in animal behavior development. From the early gain of water and land locomotion, we may add, the earliest groundwork of space and time perceptions are laid down. And this arises out of motion.

In about the coil stage the head end moves laterally in the primitive swimming movement from side to side and primitive photoreceptors appear which are sensitive to changes in illuminance.

It is important to note that much later when the brain has already been highly developed there are no nerve condutors from the photoreceptors to the brain. The order of development is centrifugal. The nerve cells of the brain push out their dendrites to the retinas to complete the linkage. Coghill tells us that often times a conduction circuit is completed by the polar extension of a dendritic fiber for as little as a hundredth of a millimeter. Consider the tremendous significance of this fact.

From very early in the behavior developmental history the progression is cephalocaudal. At this stage eating, swimming and walking are the primary concerns of the animal. The first crude limb movements are thus integral parts of the total behavior pattern; only later do these movements attain an individuality of their own. "The local reflex of the arm is not a primary or elementary behavior pattern of the limb. It is secondary, and is derived from the total pattern by a process of individuation".

This fact fits perfectly the concepts of gradients, dominance and isolation of parts, and individuation through differentiation and dedifferentiation shown by the work of C.M. Child and his students, and by Paul Weiss and others and leaves little or no room for questioning.

The limb arises in absolute dependency upon the trunk. It can only act as the trunk acts. And the struggle for 'freedom', which it finally attains, comes only with increased metabolic growth, development and differentiation of structure. When this 'freedom' becomes partially complete, it has been designated as a reflex and as a unit of behavior. It is, said Coghill, a "derived product" and hence we must assign this 'freedom' and not the reflex to the role of the primary behavior building block.

The very earliest limb movements are postural reactions, and these lower the thresholds of the skin, eyes and other exteroceptive mechanisms. The limb after 'freedom' is able to respond very precisely to proprioceptive stimuli coming from the body as a result of a particular posture <u>before</u> it can respond to stimuli which originate from the outside world. Local reflexes thus are first or primarily related to posture. In fact it was shown that the nerve supply has extended out to the regions of the limb muscles <u>before</u> the muscle tissues are formed. Growth and development are centrifugal, not centripetal.

The trunk is the pace setter, and Coghill showed further that when the animal matures and can walk, this "does not come about through the co-ordination of local reflexes of the limbs".

The whole course of behavior development in amblystoma is a continual and progressive expansion of a perfectly integrated total pattern, present in the fertile ovum, and differentiated and individuated into sub-patterns through the dominance of metabolism and locomotion.

Coghill pointed out that the physical processes of membranes (e.g. the semi-permeability and selectivity and interfaces) are also involved in the dynamics of polarity establishment in the developing nerve cells and in the origin of conduction circuits. Polarization is an aspect of metabolism and a directive force in nervous and behavior development.

The overlap in the development of preneural and neural processes shows that the accepted notions of nerve conduction do not give a complete or satisfactory account of how the nervous system operates in the development and differentiation of behavior. This point is crucial for almost all existing theories of learning.

After a nerve cell has taken on a definite and specific role as a conductor in any functional mechanism, it grows for a relatively long time in a strictly embryonic manner and in so doing greatly extends its sphere of action. One of my teachers many years ago said that if you die a 'natural' death at the age 90 you take with you millions of undeveloped neurones into your grave.

Cells grow according to a definite pattern which is set in the main dimensions <u>before</u> nervous functioning, excitation or exercise begins. So Coghill concluded that nervous function or exercise cannot be, directly or indirectly, the cause of growth or differentiation of the neurone. This is a native potential of the cell.

After the cell begins to conduct it continues to grow and to seek out new fields to conquer. Growth often means only miccroscopic lengths, say a hundredth or so millimeters, with consequent manifold changes in the original behavior of the organism. In the human foetus, for example, mass acts come first and become individualted in to specific movement patterns later with growth, development and particularly with differentiation. Perception develops in the same pattern, from the general and non-specific to the particular and specific.

The above biological discoveries Coghill regarded as facts "of incalculable significance". The notion that neurones grow early then cease and become simply a conductor in a fixed mechanism is "erroneous and wholly inadequate" to account for how the nervous and effector systems operate when we learn a sensory-cerebro-motor-feedback act. If, as Coghill states, "the neuromuscular structural relations that make the act possible must exist before the act is performed" then learning clearly must not be a mere matter of trail blazing in the central nervous system or reinforcing a conduction circuit.

There is always the biological expansion of the total organism as a perfectly integrated unity. From this there is also the individualtion of sub - or partial systems which through training or experience acquire more or less discreteness. Coghill is very explicit in telling us that "there is no direct evidence for the hypothesis that behavior, in so for as the form or pattern is concerned, is simply a combination or coordination of reflexes". On the other hand there is conclusive evidence of a dominant and pace-setting organic unity from the beginning. The maintenance of the integrity of the individual as a whole is the most elementary function of the nervous system.

There is always a surplus of nervous mechanisms in the brain and cord over those involved in discrete behavior acts. So it was concluded that the individuation of local reflexes is anticipated by the growth of nervous mechanisms, with respect to sub-patterns, long before the partial pattern or reflex appears in the animal's behavior.

One of the most important conclusions from Coghill's work was the principle that the greater the behavior organization potential of any organism, the greater the central neural mechanism runs ahead of the effector or motor mechanisms in development.

But the motor system sets or determines the forms of the behavior patterns -- and has "specific reference to particular behavior forms of the future." There is a mechanistic equivalent for man's ability to develop attitudes that can come to expression only in future behavior. This was proposed both by Coghill and C. J. Herrick and designated by them as the principle of "forward reference."

Habit mechanisms could be perfected in high degree "without the participation of the cerebral cortex." This fact was explored much more in detail by the important researches of K. S. Lshley, which will concern us next.

Coghill showed that conduction " cannot fully account for the role of the nervous system in behavior." The basic determinant is the growth potential and this is the creative function of the nervous system, both as to the form of the behavior acts and as to their control.

"Man is, indeed, a mechanism, but he is a mechanism which, within the limitations of life, sensitivity and growth, is creating and operating himself."

Our debt to G. E. Coghill is great.

VOLUME 32 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright 1960 OEP Foundation Inc. (Nonprofit) Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimentol psychology at Ohio State University, supplying optometrists with voluable procedures for use in the proctice of functionol optometry.

August - 1960 VISUAL PSYCHOLCGY Series 20 No. 11

Before we attempt the task of summarizing the important contributions of Karl S. Lashley to our problems, let us first review some of the important facts known about the nervous system and its constituent cells.

The molecular processes of the some eleven billion nerve cells which comprise our nervous systems carry their functions on over into the molar processes of movements and meanings. These are two phases or episodes in a single organic system of energy collection, storage and distribution in living organisms. This is the concept of isomorphism. It is an organismic or field approach. It cannot accept the reflex of Sherrington or the conditioned responses of Pavlov or of Hull, or of whatever "reinforcement" means, or of any form of association or connectionism as the basic mechanism of behavior.

As organisms grow, develop, differentiate and de-differentiate, as has been amply shown by many workers, the nervous system becomes more complex and behavior becomes less and less 'spinal', less reflex, less "nickle in the slot" mechanical and so less and less predictable in terms of any aspects of external stimulation or nerve conduction. The complex behaviors called perceiving, learning, remembering, thinking, creating, discovering, cannot be reduced satisfactorily by analysis to summations of separate, independent or colligated 'reflexes'.

The arguments in support are found in many recent developments in science. In insects 'spontaneous' nerve activity is reported by entomologists to be almost universal. It is impossible to reconcile this fact with the further fact that an insect's behavior in <u>its</u> geographical environment has to be adequate and appropriate or it soon ceases to exist.

Living cells, particularly nerve cells, contrary to the stimulus-response commonly held doctrine, never reach a static equilibrium or dead-center in the give and take exchange of energy with their surrounds.

The steady state of physics, in which input equals output, simply does not occur in living, growing, behaving animals.

If an animal, say, with a positive phototropic response became motionless and remained so after reaching a maximum point in the gradient of stimulus light intensity, its behavior would approach zero. This does not occur. If it does it is only momentary since any movement of the trunk changes the positions of the light receptors and so changes to some other gradient of illuminance, and so induces a change in behavior.

The general concept for this on-going activity of living cells is called <u>kinesis</u>. Lorenz and Tinbergen, who saw the universality of kinesis in all living cells, called this basic approach to the study of behavior <u>ethology</u>, a new division of science, and one directly aligned with the problems we have been considering as to the relations of the nervous system, its growth, development and functioning in the behavior of living organisms.

Lorenz designated as <u>appetetive</u> or search behavior the restless, unoriented behavior which is the objective manifestation of mounting drives. If thirsty, the animal seeks water. He may not "know" this except as a vague feeling that something is not right. The seeking, exploring search exposes the organism to a varied series of differing sensory stimulus impressions, some one of which may produce the consummatory act of drinking and the reduction or stopping of the drive for water. Or the urge may be for food, sex, play, companions, etc. When this consummatory stage is reached perceptual and memory after effects form and transform in space and time subsequent adjustory movements of similar or of the same kinds. These processes need be none other than the pattern of the delayed reaction with the concomitant signal-

ing processes which conduce to foreshortening, sensory-motor condensation, and the like.

Sensory stimulation plays a dual role. First and most primitively it partly controls tonus and the general level of excitability of the organism. Second, it furnishes, through out the several modalities, signals for search and meaning-gaining behaviors which lead to homeostasis or some satisfactory adjustory relation, biologically and psychologically, between the organism and its behavioral and geographical environs.

Kinesis is not a resultant of sensory stimulation alone. Pantin, studying the kinetic continuous activity of coelenterates, concluded that "whatever the origin of the continuous (search) activity it appears to be inherent in the animal and not initiated by causal external stimuli."

Roeder has pointed out also that the reduction of the total sensory input may actually increase activity, as is the case with nocturnal animals, and that the removal of major sensory ganglia in insects may increase kinesis until it becomes continuous.

Culler showed the same effects in his decorticate dogs who paced constantly back and forth in their pens, reminding one of Walt Whitman's "Leaves of Grass", "out of the cradle, endlessly rocking."

The vertebrate heart shows spontaneous or endogenous activity since it continues to beat regularly when nerve connections with the rest of the organism have been severed. Spontaneous activity is also characteristic of smooth or white muscles and of ciliated epithelium. Roeder, a physiologist, puts it this way: "Effectors which do not depend on a stimulus-response mechanism for their normal activity are widespread and are in no sense physiological anomalies."

E. D. Adrian, in 1930, first described the asynchronous discharge of nerve impulses in the isolated nerve cords of the caterpillar and beetle. He showed that each neuron may discharge quite regularly at its own frequency. In saline solution with glucose and oxygen the cockroach nerve cord continued spontaneous discharge for 50 hours. It was shown to be very sensitive to certain drugs and to the chemistry of the surrounding fluids, more so than to synaptic conduction in the same nervous system.

Isolated forebrain of the frog continued to discharge rhythmic waves at a frequency of 6 per second for three hours after removal.

In 1939 Gerard and Dunbar showed that the normal spontaneous rhythms of the cat geniculate body, especially the dominant one at 3 per second, are independent of the impulses reaching the neurons from the optic nerve, cortex or brain stem. The background level of excitation, determined largely by optic impulses, however, strongly influence their character. The slow rhythm fades out after hours in the dark, and is re-initiated after brief illumination.

In the cat cortex Kristiansen and Courtois found rhythmic activity closely allied to the EEG alpha rhythm, and further that cortical activity is quite sensitive to slight chemical changes in the blood.

Bremer studied the cortical activity patterns and held that cortical activity depends upon synchronized pulsations of the cells like those mentioned in the case of spontaneous activity in insects and "not upon the closed reverberating loops of nerve cells" which Eccles, and others, have claimed.

The 'resting' state of nerve cells is in reality an active state. The all-or-none law is no longer a "law". Spontaneously active cells consequently have no true thresholds because excitability constantly changes from complete refractoriness at the time of discharge and shortly after to maximum excitability after recovery and before the next discharge. So-- one may ask, what <u>is</u> a psychophysical threshold? How can one measure it?

Any reaction must be some algebraic sum of the intensity of excitation and the sensitivity or excitability <u>at the moment</u> of a cell or group of cells. What nerves can do and actually do at any instant is thus as much a matter of the 'constant and correct' ionic envirnonment as any form of external stimulation. This is why even if one could know the exact intensity and duration of the proximal stimulus, how possibly from such fact alone could one predict the response?

Kohler's work on positive and negative time errors in judging lifted weights is a case in point. And, as Miss Anastasi showed, it make a large difference whether on compares the 'standard' stimulus to the variable or the variable to the standard, disregarding for the moment the mathematical question as to just what rigorously is a standard, that is an invariant, and what is a variable?

No matter what happens in receptors or in the central correlating and coordinating mechanisms of the nervous system, the important things for behavior, broadly defined, is the thing of primary concern. It is fashionable today to talk in terms of models, analogs, computers; of coding and decoding, and so forth. But the basic facts remain as to the real functions of the nervous system, how it works, and how it is related to the things we do.

This is what Karl S. Lashley spent his active life studying, between 1890 and 1958. We turn next to his contributions.

Copyright 1960 EP Foundation Inc. (Nonprofit) Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

September - 1960

Series 20 No. 12

Our last three or four papers have attempted to show that during about the first third of this century we were in the midst of an important series of biological and psychological developments. These were the inevitable consequences of the scientific advance upon the problems of the first order importance in the understanding of the nervous system and how it operates in human behaviors. Recall that the nerve cell was not discovered until 1892 and it was not until 1912-14 that anyone had any real knowledge of the nature of nerve 'currents.'

When one surveys the past to gain the necessary perspectives for judging present theories and practices it is a large undertaking. Thus far we have cited only a few of the outstanding and important contributors and their contributions.

Karl Spencer Lashley was born in 1890 in Davis, West Virginia. Thus he was two years old when the neuron was discovered. He graduated from high school at the age of 14. He went to the University of West Virginia, from which he graduated in 1910. He then went to the University of Pittsburgh where he received his M.S. in 1911. His interests were both in zoology and experimental psychology. He then went to Johns Hopkins where he worked with H.S. Jennings, Adolf Meyer, S.O. Mast, Shepard Ivory Franz and John B. Watson, and completed his work for the Ph.D. there, majoring in zoology, in 1914. Professor D.O. Hebb, in his biographical paper on Lashley in the March 1959 American Journal of Psychology, wrote that "it seems likely that history will see him in something like the same light as he was seen in 1930, when he stood head and shoulders above the field." Cobb has remarked the attention paid by neurologists to his work; Walshe considers that the Nobel prize has been won by lesser men.

He worked at the Universities of Minnesota, Chicago and then Harvard and from 1942 to 1955 he was director of the Yerkes Laboratories of Primate Biology at Orange Park, Florida. On August 7, 1958 he suffered a heart attack and died in Poitiers, France, ending what his student and co-worker, D.O. Hebb, said was "perhaps the most brilliant career in the psychology of this century."

Lashley's contributions dealt with a number of things, some of the most important of which we shall attempt to summarize briefly here. Professor Boring wrote in his introduction to the book <u>The Neuropsychology of Lashley</u>, by Beach, Hebb, Morgan and Nissen, McGraw-Hill, 1960, that "he did so much more than anyone else in this third of a century to bring cerebral neurology away from the neurology that applies to the periphery of the nervous system and to bring it toward a field dynamics of the brain."

Among the problems to which he made important contributions were: localization in the brain regions, the neurological basis of learning, the functional anatomy of the brain, visual pattern perception and language and thought in man, memory traces, and the theory of the organization of behavior.

From 1926 to 1929 Lashley and his students trained rats to run mazes of various degrees of complication. They then made lesions in various parts of the brains of the animals to study the problems of the localization of the maze habit; on the effects of the lesions on various sensory functions; and the effects of these various lesions on the abilities of the animals to retain and re-learn the habits of

maze running.

I recall more than two exciting hours when I first met Lashley in 1929 and he expressed interest in a paper I had published on the effect of reversing the pattern of transit in pursuitmeter learning with humans. I got no differences in time and errors with humans when the pattern of movements were reversed from that originally learned. This interested him greatly because it fitted perfectly his conclusions from his maze experiments with rats, and told strongly against the then popular connectionist theories of habits in men and animals.

In 1929 Lashley published a monograph, Brain Mechanisms and Intelligence, Chicago University Press.

Briefly here, contrary to the prevailing theories of Sherrington, Kappers, Thorndike, Pavlov and others as to synaptic resistances, discrete conduction paths, biotaxis, and the like, Lashley and his students concluded that "the learning and retention of habits are not dependent on any finely localized structural changes within the cerebral cortex. The results are incompatible with theories of learning by changes in synaptic structure, or with any theories which assume that particular neural integrations are dependent upon definite anatomical paths specialized for them. Integrations cannot be expressed in terms of connections between specific neurons."

Further, "the contribution of different parts of a specialized area or of the whole cortex, in the case of non-localized functions, is qualitatively the same. There is not a summation of diverse functions, but a non-specialized dynamic function of the tissue as a whole." "The analysis of the maze habit indicates that its formation involves processes which are characteristic of intelligent behavior, hence the results for the rat are generalized for cerebral functions in intelligence." "The mechanisms of integration are to be sought in the dynamic relations among the parts of the nervous system rather than in details of structural differentiation." It was further concluded that the maze habit "cannot be interpreted as a series of kineasthetic-motor reflexes but must be referred to some intraneural mechanism capable of producing an integrated sequence of movements in the absence of directive sensory cues." The extirpation experiments proved that when only 40% of the cortical mass remained the previously learned habits still remained. This led to the statement of the Lashley principle of equipotentiality.

To the question: Where are psychological functions localized in the brain?, Lashley said that after many studies on rats and monkeys, "we gained a meaningless answer." This can only mean that if one stimulates with a gold needle and the subject responds to electrical excitation of a small local region but remembers the sight of an electric sign or what not, there is no evidence from this fact that the memory and recall is merely due to the activation of a small local group of brain cells. The energy spreads out in gradients, and any notion of highly specialized localization, among other things, completely disregards the very necessary contributions of the feedbacks from the effectors and the transforming power of these pace-setters.

In 1934 Lashley published a paper on the perception of spatial extent and depth in the visual field. Are these things learned by experience or are they the product of growth functions in the absence of visual stimulation adequate for learning? To answer such questions Lashley developed his 'jumping' apparatus. The animals stood on a platform and in order to eat, had to jump to another platform back of which two doors bearing visual designs signifying food or no food were placed. Thirteen rats were reared for 100 days in total darkness, then were trained to jump across a space of 20 cm. from the starting platform to get food. The platform was arranged to measure the force expended in jumping. The distance of the jump ("over the clift") was increased and comparisons were made with those of animals reared in the normal visual environment. Lashley concluded from his results that "the visual perception of distance and gradation of the force of the jump to compensate for distance are not acquired by learning, but are the product of some innately organized neural mechanism" (J. Genetic. Psychol., 1934, 45, 136-144).

This result was in agreement with the earlier work of Helen Frank, in Germany; with that of Revesez, and others. No mention of any of these is found in the 1960 paper in the Scientific American on the "clift" behavior by Mrs. Gibson.

Numbers of experiments were made by Lashley attempting to answer questions about the functions of the visual cortex and the equipotential regions of the brain. "Part of the occipital lobes were destroyed, in a variety of combinations.... So long as some part of the anterolateral surface of the striate cortex (the projection field of the temporal retina corresponding to the macula of primates) remained intact, there was no loss of habit. Any small part of the region was capable of maintaining the habits based on discrimination of intensities of light."

So we must conclude that the problems of psychophysics, that is of stimulus intensity and response magnitude, has no constant or invariant correlate in the 'visual' cortex.

In other experiments, Lashley showed that "discrimination of visual figures could be learned when only one-sixtieth of the visual cortex remained." The striate bodies, the large relaying and distributing nuclei before impulses reach the cortex, were of the highest importance, and that "the spatial reproduction of sensory surfaces upon the cortex becomes intelligible only on the assumption that it forms the basis for some 'field organization' in which the pattern of excitation in definite spatial relations is a determining factor in the arousal of the final motor response. These things were discussed by Lashley in a paper, Mass Action in Cerebral Function, Science, 1931, 73, 245-254.

Taken in conjunction with the work of Van't Heuven in 1925 who showed that the projection of retinal excited areas to areas 17, 18 and 19 are in no sense geometrically coordinate, it is (or is it?) surprising to hear and read so many statements about 'fusion centers,' retinal images and their direct projection to the occiput, etc.

Lashley figuratively threw a large monkey (and rat) wrench into the physiologizing of behavior. There remains some further important matters of his to consider next time.

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1960
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

October - 1960 Series 21 No. 1

Approximately fifty years of Karl Lashley's active life in making experiments was spent in studies of fundamental questions as to how the brain and nervous system 'works' in those types of behaviors called learning, perceiving, remembering, thinking, talking, and the like.

Early in his career Lashley came to the conclusion that the then widely accepted tenets of the out and out mechanism of association by sheer frequency of repetition, or of conditioning by whatever means, were not satisfactory accounts either of the events in the brain and nervous system or of the phenomenal experiences classed as perceiving, learning, remembering, etc.

The biological and psychological objections raised by Coghill and many others found brilliant confirmation in Lashley's results. Connectionist theories, of whatever sort, based on specific and invariant conduction circuits from the sense organs to central distributors to the effectors and feedbacks, could not be demonstrated to be the neuro-physiological basis of the psychological functions mentioned above. How, under such a theoretical framework, could Newton and LaPlace have invented the calculus? How could the value of pi have been discovered?

In 1932 Professor Boring published a paper in Science in which he pointed out that in attacking these problems the physiologist holds that the "brain, made up of neurons, is capable only of that excitation which is the sum of the excitations of many neurons---the central neurons obey the same laws and are excited under the same limitations---as the peripheral neurons which have been experimentally studied. Physiologists---oppose another belief, that the organization of cerebral excitation corresponds to the organization of phenomenal experience." 'Corresponds' here could mean that both the molecular nervous processes and the molar experiential or psychological processes are isomorphically mere phases of a single unitary reality. This was the position taken by the Gestalt theorists, Wertheimer, Köhler, Goldstein, Koffka and others. It accords closely with Lashley's findings and conclusions.

In the years when Lashley was most active it was quite common, fashionable, and "respectable" for students of behavior problems to seek for physiological and neurological answers to their problems. Then Lashley came along and showed conclusively that the answers to psychological problems could not be found in physiology. But there were Lashley's experimental results. This meant that pet theories based on unproven physiological evidence had to be abandoned and something new discovered to take their places. This is always hard for many people to do.

Let us look at a few of Lashley's more important contributions. In visual pattern discrimination, using his jumping technique, "the entire right and a large part of the left striate area of the visual cortex was destroyed in each of a series of rats and the animals were tested for detail vision. Discrimination of visual figures was observed in animals with not more than 700 neurons in the geniculo-striate system, about 1/50 of the number usually present in each lateral geniculate nucleus." Thus "visual discrimination and learning may be carried out in a normal or near normal manner by a minute remnant of the cerebral area immediately adjacent to the lesion. It therefore argues against any trophic changes in the residual tissue as important causes of defects observed in other functions after similar lesions." So, it must be concluded that visual functions, such as acuity, form perception, stereopsis, etc., cannot be regarded as energy repli-

cations or distributions correlative to such distributions over the surfaces of the retinas.

Cortical localizations of both sensory and motor functions have largely been inferred or supposed from observations of behavior. Some relatively simple nervous activity in a restricted 'sensory' area gives rise to 'sensations,' and likewise for motor and volitional functions. "Such concepts are oversimplified and must be abandoned" said Lashley.

Regarding conscious states Lashley proposed that "there is some reason to believe that these states can be correlated only with the summated activity of all centers simultaneously excited."

Lashley agrees with the position of Kurt Goldstein, who held that the functions of any and every center are dependent on its relations to the rest of the intact nervous system. This is an out and out physical monism and one may list among its advocates: Ernst Mach, Wolfgang Köhler and most of the field and Gestalt theorists. It is not a more or less isolated activity of any specific brain region but a dynamic synthesis of the neural, biochemical and effector systems of the whole organism which constitutes behavior.

In any attempt at a descriptive analysis of such a system something like Boolean algebra or factor analysis may lead to a functional classification based on multidimensional relations and thus supplant the present unsatisfactory attempts to link such a complex function as the visual perception of size and distance to some region like Brodemann's areas 17, 18 and 19. Many people still try to do this. But the brain is an analogical mechanism, not a digital one.

No one really questions the fact that the neural and physiological mechanisms are basic to all functions; and no one can question the fact that functions can and do transform physical mechanisms.

For example, your digestive tubes are normally something like 32 feet long. If you were to go on a perfectly bland diet for two months and then measure its length you would find it much shorter, perhaps as much as 50%. Or train your own figure alternation rate up to 60 to 70 phases per minute. Compare your stereopsis before and after this training; or slow it down to 5 or 6 per minute and do the same. Will stereopsis remain the same? Assuredly not. There must be something like a homeostatic or favorable balance between the physical and behavioral environments if either is to operate to further the business of 'good' organic living. 'Good' here means a specifiable set of parameters of such and so powers in a very complex equation, which unfortunately cannot at present be written. It is, as St. Paul said one time long ago, that "we know only in part."

Lashley pointed out that central integration establishes "fields of force to determine spatial orientation and the control of the serial timing of activities. Each of these functions implies a different mechanism of organization, and consequently a spatial separation of the fields in which the different processes operate." And that "experimental and clinical data indicate that the dissociation of functions resulting from cerebral lesions is in harmony with the assumption that cerebral localization is determined by the separation of such incompatible mechanisms."

It is interesting to note that in 1927 Spearman in England proposed that 'intelligence' is a function of some masses of undifferentiated nervous energy, which he designated as a "g" factor. This accorded with the mass action and equipotential findings from Lashley's experiments. In one of these he showed that simple problems can be learned at almost normal rate by rats and monkeys after brain injuries of considerable extent, while complex problems are learned slowly if at all. It seems probable, Lashley said "that the great fragility of color vision and the perception of depth is due to the high degree of organization required for these functions rather than to their separate localization in the cortex."

Memory and memory phenomena have always presented difficult problems. Memory traces were studied extensively by Lashley. He noted that "the attempts to account for all memory by any single theory involves assumptions which are not supported by any evidence now available." Speaking a sentence or playing a musical phrase cannot be accounted for as a single chain of con-

ditioned reflexes. When the nervous system is subjected to any pattern of excitation it may develop a pattern of activity throughout an entire functional area by the spread of excitations, just as the surface of a liquid develops an interference pattern of spreading waves when it is disturbed at several points, responding perhaps in complex patterns of reverberatory circuits, reduplicated throughout the area. He further said that the same neurons which retain the memory traces of one experience must also participate in countless other activities.

Along with Klüver's results from visual discrimination experiments with monkeys following various degrees of cerebral damage, Lashley showed as did Klüver that set, attitude, self-imposed instruction, and affective and emotional concomitants were potent factors in the retention and recall of any experience, and that these things are the real determinants of the reactions called 'conditioned,' not the attributive properties of the stimulus, as the advocates of positivism still persist to insist.

Speech, language and communication form the basis of our culture and are the distinctive forms of human behavior. Lashley's handling to the neural basis of these cannot be summarized here but should be read carefully in <u>The Neuropsychology of Lashley</u> (McCraw-Hill, 1960).

How does organization in the cortical field take place? Lashley said, "within the entire cortex, for certain functions, and within specialized areas for others, the subordinate parts are all equally capable of performing the functions of the whole. Even where the highest degree of specialization exists, as in the visual and motor areas, the facts of the equivalence of stimuli or of equivalent motor responses preclude any narrowly localized specialization of intercellular connections."

As I understand it, Lashley held that organization in the nervous system, the establishing of perceptual frames of reference, or ob Bertlett's schemata, means that regions which are actively compatible or 'similar' tend to become a dynamic unity. Such homogeneous grouping of function is found mainly in what has been called set, attitude, intention, determining tendencies, meaning tendencies, all acting under the pace-setting aegis of the affective and emotive concomitants experienced in the foreperiod, often before any external stimulation.

Thus Lashley went a long way to break down the artificial and prejudicial barriers between physiology and psychology in their valiant struggles to understand and predict behavior. He was modest and constantly pointed out the large gaps in our knowledge of the fundamental facts as to how the brain and nervous system 'works.' Karl Lashley was a great scholar and scientist. Our debt to him is great, indeed.

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1960
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

November - 1960 Series 21 No. 2

The history of our culture and understanding is most interesting. One looks at the marvelous discoveries made often in short time and then one wonders why it took centuries, for example, to invent and develop such an important and useful thing as logarithms. The first microscopes were banned by the religionists because some padre looked at a drop of stagnant water and saw unknown minute living animals moving about and, not knowing them or about them, decided that microscopes were instruments of the Devil and that 'good' people should have no truck with such infamous things as microscopes!

Primitive people were curious about things and processes in nature. Oddly enough one of the last or most relatively recent of his interests was man himself. For example, it was not known until about 1865, the end of the Civil War, that he discovered that his body was made of cells. Before this tissues were seen as a reticulum of interspaces, like a woman's knitted shawl. It took 30 more years to discover the neuron, in 1892. So it is certainly not strange that before this time all sorts of bizarre theories and hypotheses had been proposed to answer the questions at to how and why we behave like human beings.

In Shakespeare's time anger was regarded as a heating of the liver, and the brain was thought to be a huge tear gland which generated cold tears to cool the heat of the 'angry' liver! Love was in the heart, of course, and we only wonder now why it took us so long to know "only in part" how we are fearfully and wonderfully made and how we behave as we do in our various geographical environs.

If you have read the last several of these papers and have been ambitious enough to summarize and synthesize them as to some facts as to how the nervous system develops and how it works in the organization of our acts into behavioral units, then we may be ready to take the next step.

It is not at all surprising that in the infancy of science description of things and processes in nature should have proceeded by the analysis of these things into their elements. Elements were the ultimate building blocks, incapable of further analysis and definable only in terms of their attributes. This was an out and out atomism and mechanism.

Was the cell the elemental unit of biological or living structure and function? By no means, even if at one time it was proposed as true. As analysis pushed on, the cytologists or cell scientists had to go to the physiologists. There they got some but not too much help. Then to the organic chemists, to the physical chemists, to the atomic and sub-atomic physicists, and finally to the philosophers of science.

With increased knowledge the answers were pushed back and back so that when seen in great magnification a cell was a most complex set of almost infinitely small particles or electric charges swirling about in orbits about a nucleus at fantastic speeds. The original 'reality' had vanished.

It became clear but troublesome to reflect that this long course of analysis was the hunt for the ultimate 'first cause' of things -- and this purely and simply was teleology or non-science.

Atomistic analysis into such things as cells, sensations, reflexes, etc., was the Aristotelian approach. You first pushed analytic description as far as possible, then you set about to discover how the separate units became <u>organized</u>

into the kinds of events described in ordinary terms as perceiving, learning to write and speak, remembering, thinking, creating new combinations, etc. This put the fat right in the fire. The only way out, it seemed, was the formulation of the theory of connectionism or association, in one form or another. All that was needed was two or more 'elements' to occur in the same temporal present. This in some unknown way made them 'parts' of a larger aggregate. It was disconcerting that some of the elements changed when they became parts of larger aggregates, but this did not seem to attract much attention.

But when rigid experimental tests were made, the magic formulas of association, conditioning, and the telegraph concept of the functioning of the nervous system was either seriously questioned or abandoned.

But if not this, then what? In contrast there were from equally early times those who took an opposite point of view or approach. These were the followers of Gallileo. In biology they held to an organismal as opposed to a cell theory. They held that analysis, dissection, fragmentation, only obscures or destroys the things one seeks to discover, namely the facts about the organic unity and continuity of the living organism. They held that the basic and fundamental realities are wholes, not parts. 'Parts' are all man made and artificial.

The view of wholism or field theory is very old. As early as 600 B.C. the Chinese philosopher Lao-Tse wrote that "The sum of the parts is not the whole." The problem of 'the one and the many' is one of the oldest in philosophy. It had been with us for a long, long time.

Things, in the Gallilean approach, are described in terms of the operations which produce them. Force, in physics, is mass times acceleration, a relation; a name for a relation, not a thing which one can reify. The attributes of the atomists could never become agreed upon. Change tone intensity and you change pitch; hence these are not independent attributes.

As early as 1905 Sir Charles Sherrington wrote that the then current concept of the reflex as the basic element in skilled acts was not true. Many scholars had objected to atomism on the ground that, for example, a melody was more than a mere succession of single tones (Ernst Mach, 1885), or a sentence was more than a mere collocation of word sounds or visual symbols. These wholes had something more. This 'something more' was nothing mystical or non-scientific. Wholes had form, structure, organization, plan, plot, design, schema and led to some form of meaning to the perceiver. These things were never revealed by no matter how extended and analytic description of the constituents. All units, they maintained, were man made, artifacts and that their properties as units derived from their roles in the wholes in which they were perceived, not from any intrinsically possessed attributive properties.

Early in the century, DuBary, a German botanist, wrote that "plants build cells, not cells plants." Speeman, in Germany, and Speidel, in America, won Nobel prizes by showing independently that cells, transplanted surgically from one embryonic germ layer to another, which if undisturbed became, an eye or a limb bud, went on in the new position to develop into what 'belonged' in the new position to make an integrally functioning organism. The die was not cast in heredity in this case. The eye <u>anlagen</u> became a limb and the limb cells an eye when transplanted into the new position in the dynamic, developing and differentiating behavior <u>field</u>.

There were other lines of evidence. There were the often quoted experiments of the Italian, A. Marina, who between 1905 and 1910 surgically interchanged the eye muscle of monkeys. He first interchanged the medial and lateral recti, and in a further experiment 'substituted' the superior for the lateral rectus. On healing it was predicted that the eye, in atomistic terms, would move out rather than in, or up instead of out. If the eye movements were in response to signals from a brain center a curious nystagmus should be seen. But, after healing, the voluntary and automatic movements of the eyes were made exactly as in the normal fashion.

The movement toward some sort of field theory thus has had a long history. Ernst Mach, in Vienna in 1885 wrote his important book, Contributions to the Analysis of Sensations. In it he called attention to the fact that geometrical optics, physiology, etc., could not satisfactorily "explain" the facts of space and form perception. If a square is seen as a square and another identical square is rotated 30 degrees the second is no longer seen as a square but as a diamond. Those who have seen my golf-course footbridge slide have seen the great importance of position and orientation for visual form perception. These things Mach called "affairs of the intellect, not of sensation." He even went further and proposed that "sensations of space are connected in some way with the motor apparatus of the eye." Mach thus opened up new lines of thinking.

There was much in the air in the early years of our present century to bring scholars to grips with the persistent problems of elementarism versus field theory. Physics was revolutionized by the discoveries of Maxwell and others which made fields and field dynamics a new and important forward movement.

About the same time, Christian von Ehrenfels, along with Meinong, Husserl and others, who were philosophers, were calling the attention of psychologists that perhaps the most important qualitative data of sensory fields had been completely overlooked in the then current attempts at introspective analysis. Sets of stimuli behaved nothing like any information brought about by anything known about specific stimulus members.

So, here was opened up an entirely new approach to the problems of the physical and physiological operations of the nervous system and how they related to the perceptual and behavioral worlds of the person. But, more about this next time.

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1960
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

December - 1960 Series 21 No. 3

In the year 1886 Earnst Mach published the first edition of his important book, "Contributions to the Analysis of Sensations." Mach was a physicist, mathematician, psychologist, and philosopher. In this book he pointed out that, "a tree with its hard, gray, rough trunk, its numberless branches swayed by the wind, its smooth,soft, shining leaves, appears to us at first a single indivisible whole." A surface rough to the touch, a melody or a spoken or written sentence was the same sort of thing.

Our perceptions, Mach held, are primarily and basically not mere assemblies of elemental parts. They are something new and to some extent independent of the subparts.

Mach did not see clearly the way out of the dilemma of elements versus field structures. He was essentially a sensationist, but realized perhaps for the first time the problem of wholes in perception...a problem which had to be met, no matter how much one was devoted to the measurement and description analytically of the component 'sensations.' In 1910 or thereabout, E.B. Titchener said that sensations are logical constructs; they are not real; they represent, he said, "a closed chapter in the history of experimental psychology."

In 1890 Christian von Ehrenfels, an Austrian philosopher, published a very important paper in an obscure journal. In it he followed the lead of Mach and concluded that the totals of sets of stimuli coming from any <u>thing</u> are different entities than the mere sum of the parts. If, for example, you are asked to sing or reproduce a melody, and you in all probability do it in a different key or pitch from the original, you do not reproduce the sum of the single tones but a new set of relations of the pitch frequencies of the originally heard melody.

In summary, said Ehrenfels, you have reproduced a <u>form-quality</u>, which he termed a <u>Gestaltqualität</u>, or unity, not the elements in sequence comprising the whole. Form-qualities had both spatial and temporal properties which contributed to the perception and memory of the whole. This he concluded was not a mere matter of perceiving psychophysical differences of pitch, time, intensity, etc. There was something about <u>the whole</u>, a self-regulating character or property contributed by the perceiver, not present in the mere primitive discrimination of differences.

This point of view, of course, started things. Some scholars agreed, others vehemently disagreed - and still do.

Ehrenfels said merely that when sets of stimuli occur in a sensory field some emergent properties develop. All of which sounds much like the emergent evolution of Lloyd-Morgan. But this is denied, for instance, by Professor Köhler who has shown in an early book (Die Physische Gestalten) that the <u>new</u> property is not an emergent in the ordinary sense, but is a dynamic development of forces and their spatio-temporal distribution in the sensory-cerebro-motor system.

Köhler points out an illustration. If soap is dissolved in a flat dish of water the liquid appears dim or turbid. But if one examines it with one eye looking through a small hole in a card, the hole is filled with a bluish or reddish gray cast. The quality of dimness or turbidity will have disappeared. One has to have a certain limiting amount of area or extent to show turbidity or fuzziness.

A Braille letter made of raised bumps, is

to my touch fuzzy and not clear. But to the trained blind reader it is sharp and clear. Most myopes see clearly and sharply through pinholes. This changes the periphery-central retinal relations, and also the hyperfocal distances of the optical system.

Köhler pointed out that color experiences are complex biochemical events. The chemist can analyze them, but such analysis tells us nothing about the perception of the color spot, which also depends on brightness, contrast, area and surrounds, etc. "We are compelled to recognize the occurrence of somewhat extended dynamic realities which would be destroyed by analysis which goes too far." This is true in chemistry, and also "when we face it in a sensory field."

Form-qualities were not clearly recognized by Ehrenfels as the products of the transitive organization in the extended field of the sensory-cerebro-motor system.

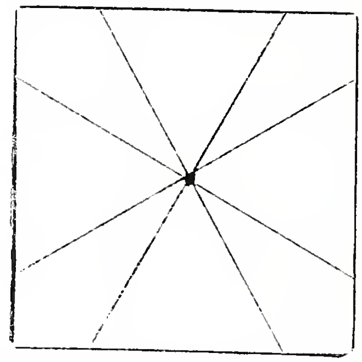
Köhler has pointed out that the word Gestalt in German not only has the meaning of shape or form as an attribute of things, but it also has the meaning of a concrete entity per se, which has or may have a shape as one of its characteristics.

Since Ehrenfels time the emphasis on his form-qualities has shifted to studies on "extended events which distribute and regulate themselves as functional wholes," quoting Köhler, and "such processes will have certain characteristics which they possess only as extended states, and.... the same will hold for their parts." So, field dynamics applies not only to sensory processes, but to perceiving, learning, remembering, thinking, feeling and acting. It is a fact in physics, chemistry, biology, all divisions of science.

How is it possible for me to see a circle as a circle? Is the 'retinal image' a faithful projection of the stimulus circle? The answer is no. The retinas are anisotropic. The spatial separations of the superior, inferior, nasal and temporal quadrants are quite different. Only because we can structure visual impressions in terms of active touch and movement and their after-effects can we see why the dinner plate of my neighbor at the table in the oblique to my vision is not an ellipse but a circular plate or dish.

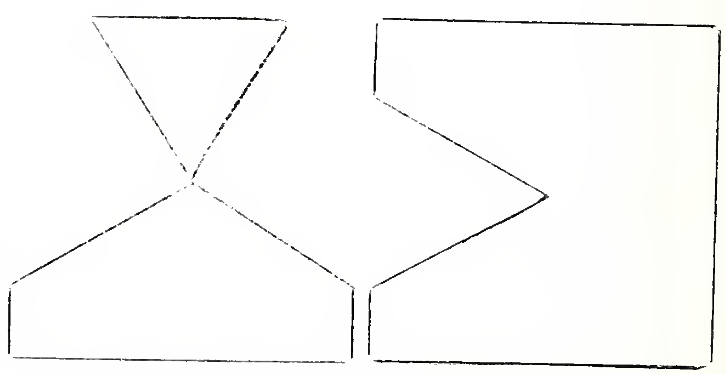
In 1894 Dilthey, a philosopher, followed Ehrenfels observations and arguments and proposed that the perception of the whole is a condition precedent to the truthful or adequate interpretation of the 'item,' element or sub-part. The units or constituents are what they are because they belong.

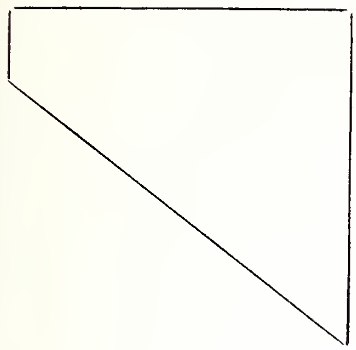
In 1921 Edgar Rubin published his pioneering results from his studies of visually perceived figures. He showed how figures are formed and the dynamics of figure-ground relations. If you look at the following figure you probably say it is a four blade propeller in a square.



If you continue looking this soon changes and you see four larger blades. The original figure becomes ground and the ground becomes figure. Graham showed that if the angle of the small blades is reduced that the small figure stays longer before giving way to the larger.

It is not likely that you saw any or all of the other shapes or forms in the original figure above. Here are a few.





So, what one sees, must be a matter of perceptual organization, partly determined by the space-times relations of what is out there and partly what goes on in the dynamic fields of the body of the perceiver. This is figure-ground structuring, disembedding, etc.

Skill in doing these things, is in part, learned, and in part determined by the facts of our body structure, design, etc. The work of Gottschaldt who studied extensively the role of 'past experience' led him to conclude that experience or training alone is not enough to explain the processes which actually occur in perception.

About 1912 Max Wertheimer in Germany reported his observations on visual apparent movement phenomena. He could not explain what he saw in terms of the then current neurophysiology or geometric optics. He attracted Köhler and Koffka to work with him. This trio, is regarded as the real founders of Gestalt theory in psychology. Wertheimer studied what he called the phi effect, where two lights not too widely separated flashing on and off successively are seen as a single light moving back and forth. These men showed that there is no difference, so far as 'reality' of motion is concerned, between the apparent movement and the real movement of a single light.

In 1944 Köhler and Wallach published their monograph on Figural After Effects - An Investigation of Visual Processes (Proc. Amer. Philos. Soc., 1944, 8, 269-357.)

This was an extensive inquiry into the phenomena first described by Professor E.B. Delabarre and many years later by J.J. Gibson.

If the center of a curved line is fixated for an inspection time of 20 to 30 seconds to a couple of full minutes, then the curve is removed and a straight vertical line is passed through the point of steady fixation, the straight line appears curved in the opposite direction to that of the inspection curve. The after effect has been shown to last for hours or even weeks and perhaps longer in particular instances. Three dimensional inspection figures gave better after effects than planar ones.

The conventional notions of nerve conduction could not 'explain' these effects. Köhler studied extensively the working of the brain in such instances, using a very excellent EEG apparatus. He proposed and showed that figure processes, including figural after effects, are produced by direct currents in the brain -- an electrotonic effects. This was something entirely new and broadened the conception of the problems of perceiving, learning, etc. It sets for us many problems for research as to how these DC potentials can be controlled and how they operate in specific cases of perceiving space, time and motion, things, forms, memories.

Gestalt theory has been controversial, of course. But none can deny its importance.

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1960
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLCGY

January - 1961

Series 21 No. 4

In the immediately previous papers we have considered some problems of the relations of the development of the nervous system to the development of behavior. This was done for a fundamental reason. The work of Coghill, Child, Weiss, the Herricks, Hoagland, Lashley and many others has presented for us the picture of the organic unity and continuity of growth and development.

From the fertilization of the ovum to and beyond adulthood the development of the organism as a living and behaving unity proceeds as a consequence of metabolism. The developing organism forms dynamic gradients about and within the central stem or trunk. This integrates all movement systems and sensory signaling systems <u>outward</u> from the trunk. The controls are central, not peripheral.

The nervous system comprises an enormously complex three way integrating, correlating and coordinating mechanism. First the old nervous system - the autonomic, sympathetic; the central system with its eight or nine thousand million nerve cells; then finally the chemical conduction and control system. All three of these are really one functionally, and extend out from the trunk to instigate activities in the effector organs and to bring in 'information' both from the body and from the geographical world in which the organism has to live, move, and have its being.

Up to the early years of this century there was not and could not have been this concept of the relations of the nervous system and behavior. The truth telling researches had not been done. Those who looked at the problems from the analytic or cell theory point of view got back to 'reflexes' as the basic building blocks of behavior. And when instances of behavior were seen which manifestly could not fit the stimulus-central process-response pattern of the 'reflex' doctrine, there was the convenient if not very satisfactory hamper called instincts into which all such troublesome problems could be dumped.

But we live and learn. In the first couple of decades of the century evidence which could not be disregarded was rapidly accumulating favoring the organismal viewpoint in contrast to the cell theory. This was not only true in the biological divisions of science but particularly with the experimental and theoretical psychologists who were struggling with the problems of why things look, sound, taste and feel like they do; how we perceive forms, space, time, motion; how we learn, remember and forget; how we can plan, solve problems and invent new things.

In the first third of the century questions were raised about the 'reflex' by men like F.M. Alexander and John Dewey. Dewey held that stimulation, for instance, lay inside and not outside the act.

He cast serious doubt that skilled manipulatory acts could be nothing more than series of 'reflexes.' Finally, about 1943 J.F. Fulton stated that "the central nervous system is organized, not in terms or anatomical segments, but in movement patterns." Movement patterns could not be accounted for in terms of fixed conduction paths from the sense organs, through the central mechanisms and out to the effectors. Likewise perceptions could not be reduced to groups, sets or patterns of elemental sensations. There has to be something more. This something more was nothing mysterious or spooky. A melody was not a mere succession of single tones. The organism could and did organize, structure and pattern the incoming impressions and the intra-organic ones according to what

was called a schema, plan, motif, design. Much of this complex set of processes came not from external stimulation but from memory trace systems, sets, attitudes, postures, intentions, desires, and the like. Most of these were 'silent processes' and were not available for introspective examination and report.

The growth of information as to fields and field dynamics led inescapably to a critical re-examination of certain basic concepts.

Skilled manipulatory, movements or acts could not be analyzed into patterns of, or concatenations of, reflexes. A complex series of moves learned with the right hand and arm could be performed almost as well with the left side which had no training or with the muscles of a leg. The organism was not a set of levels. It was not a stupid machine. It could perceive, understand. It could find other ways of bringing about some desired end if the one learned had, from some reason or other, been blocked. The dualism of perceptions and memories as mental events and sense organs and brain processes as physical events became for many no longer useful or defensable.

Last month we attempted to point out some of the antecedents which led to the formulation of the field or Gestalt theory in psychology. As Köhler has ably shown these are developments not only in psychology, but in all divisions of science.

To understand this point of view we must examine some fundamental postulates.

As I look out of my window I see a crab-apple tree. Most of the leaves have fallen. A few small red apples still cling to the branches. Red apples? How can an apple or a book be red? Red is a word. It is a name I give to the fact that sunlight composed of all visible wave lengths falls on the apple. Because of its chemical structure it has an absorption spectrum which only permits the long wave lengths to be reflected. These come to my eyes and only a few photons are needed to touch off the unknown changes in the cones which transform the light energy into another different kind of electro-chemical energy - nervous energy. The slow moving, virtual transference of ions moves in over a relatively small number of conducting fibers where it fans out in gradients of potentials involving millions and millions of neurons and finally results in some effector processes which feed back afferent impulses into the central integrating and distributing mechanism. This is a closed or homeodetic circuit, in contrast to the open loop concept of the reflex.

The 'red' apple is the <u>distal</u> stimulus. It reflects light of such wave lengths, intensity, area and duration that processes are set up in the perceiver which may cause him to say that the apple is red, not green. The eye, or any sense organ, is thus a transformer and it functions to convert the energies of the distal stimulus and make them over into the <u>proximal</u> stimulus, which is the pattern of afferent energy sent to the brain. So, to ascribe the quality of redness to the apple, the distal stimulus, is to commit the stimulus error.

To show how one cannot find an easy answer to the questions one may raise about how one sees by no matter how detailed a description and analysis of the distal stimulus patterns let us look briefly at the Liebmann effect.

In 1927 Miss Liebmann placed an irregularly shaped blue patch upon a neutral gray background. She then brought the luminosity of the figure nearer and nearer to that of the ground. At a very near point the figure lost its sharpness and definition. Its shape became simpler. Finally when the brightnesses were equal the shape was completely lost. Only a vague and vacillating blotch was seen and this completely disappeared for short periods of time.

The Gestalt contention was that color seen in any part of a field depends upon figure characteristics of this region. Color is as much a matter of brightness and contour as it is one of purely wave length.

One does not therefore 'see' the distal stimulus. One sees the net intra-organic processes which accrue and follow upon the effects produced upon the organism by the proximal stimulus and the feedbacks from the movements, implicit or overt,

which must follow or there is nothing. The common denominator of meaning is kinaesthesis, said Titchener many years ago.

From a number of such kinds of experiments the field or Gestalt theorists formulated one of their most fundamental postulates. The light from the red apple, the photochemical retinal processes, the brain, effector and feedbacks are all molecular processes. The perceptions of form, size, color, brightness, position in space, etc., are molar processes. Molar processes are molecular processes in extension. They are two phases or aspects of the same reality. They are one, not two, things. This is the principle of isomorphism.

If one thinks in such terms what happens, for instance, to the fundamental problems of psychophysics? Of learning, memory, perception, problem solution?

No one can question molecular facts. No one can question molar facts. But molecular facts are not independent events. Rather we must think of them as processes within and determined by larger dynamic field events.

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright 1961 OEP Foundation Inc. (Nonprofit) Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

February - 1961 Series 21 No. 5

Last time we pointed out that when a sense organ receives an impact of energy which activates it, the work of the sense organ is that of a transformer. It is also a compensator. The ear for example operates so that when the sound is louder the drum relaxes and so tends to keep intensities in the median range. Weak sounds cause the drum to tighten and so to intensify them.

When the eye is stimulated and a few photons reach a retinal cone, most of which is non-nervous tissue, photo-chemical processes are instigated which result in a transformation of the light energy through very intricate biochemical and physical processes into afferent nervous energy. This moves in to the central stem and spreads out through millions of neurons in the brain and some effector processes follow. From these, afferent feedback impulses come back to the correlating and coordinating regions of the stem, making a closed-loop conduction system. All these are called molecular processes.

But also from a few milliseconds after light first reaches the retinas the observer 'sees a light.' It was to him round and green. Soon it changed to yellow, then, to red. He backed his right foot off the throttle pedal and pressed down his left foot on the brakes and brought his car to a stop. He was already a few minutes late for an appointment. Inwardly he resented the red light which interrupted his movement system. But his memory traces told him silently that he must stop. All these acts of perceiving, remembering, feeling, talking, acting, are designated as molar processes. The 'stimulus-out-there' does not contain them. It only triggers off processes set and ready to go at the appropriate time. The light was the 'thing-out-there.' When it changed wave-length it set off molar processes 'in here.'

It has always been troublesome and confusing to study human behavior by postulating, explicitly or implicitly, the dualism of the 'out there' and the 'in here.' When one studies the 'out there' things, lights, sounds, temperatures, tastes, etc., he operates as a chemist, physicist, physiologist, neurologist, etc. He measures and describes them in specific units and discovers relations among them. His units are invariant. He deals, as the late Professor Titchener put it, "with the world with man left out."

When one studies the molar processes, the percepts, memories, feelings, judgments, etc., which the organism as a unity does <u>before</u> <u>and</u> <u>after</u> external stimulation, this is psychology. It deals with the world "with man left in."

To say that one approach is scientific and respectable and the other is not is simply pure unadulterated ignorance and nonsense.

Last time we showed that field theorists in psychology, like Wertheimer and Köhler, had proposed the principle of isomorphism. This says essentially that molecular and molar events are not two things, but one. Molar processes, they said, are only molecular processes in extension. Extension in time and space.

This is only another way of saying that the real, basic and fundamental thing in nature is <u>unity</u>, achieved by the organism and its environs or surrounds. Suppose for instance, that suddenly there was no longer any gravitational environment, or no oxygen in the air or water. It is evident that you are not bounded by your skin. Just as soon as disjunction occurs, within and without the organismal organization in nature, you are in real trouble. The

high microptic person who drives in modern traffic is 'accident prone.' Any disjunction in the relations of the 'out there' and the 'in here' sets the stage for behavior troubles.

It is said that it has taken about three hundred million years to evolve and develop our present organic form. Only very recently, in this tremendous time continuum, have we worn clothes, lived in houses, cooked our food, developed spoken, written and printed languages (something like 2,500 different ones!) invented systems of numbers for counting, measuring and computation. Only minutes ago, in the grand time continuum, have we become civilized, whatever that term means.

There have been many scholarly studies of the lives and behavior of our primitive ancestors. In some parts of the world today there are primitive groups living in much the same way the cave men did.

Buytendijk, the biologist, pointed out that "in the whole animal world the correlation of the animal and environment is almost as intimate as the unity of the body." In primitive man, his perceptions of 'things-out-there' was something quite different from that of modern civilized man. For him a chair was 'something to sit on.' So a box or a tree stump also belonged in the class 'chair.' Things in the world 'out there' were for him 'things-of-action' or signal-things. He was stimulus-bound. Their valence or demand-character was a perception of what for him was the expedient things for him to do about them. What things meant for him was determined by his own actions. But these actions were almost wholly functions of the properties, attributes of the externalia. Appurtenance was, of course, also an important factor. If he was tired the chair was to sit in; if he was vigorous or playful its signal-value was something different; and if he was in great anger it could become a lethal weapon.

Volkelt put it this way, "The perceptions ... (an animal and primitive people) exist only in so far as they are part of a wider totality of action in which object and inner experience exist as a syncretic, indivisible unity."

In several experiments, one by De Jong, dogs were trained to open a door to food by pressing down on a small horizontal board or treadle. As soon as the habit was thoroughly formed the position of the board was changed to the vertical. The dog was helpless. He 'knew' the object only in terms of his immediate and specific reaction to it in the learned situation.

Thus it was apparent that both in animals and primitive people the coordination of appropriate movement and sensory impression is essential and fundamental for form perception.

For the primitives 'things-out-there' were 'things-of-action.' Their meanings were in terms of what the perceiver did about them and with them, not in terms of any appurtenance to conceptual frames of reference.

In certain types of brain damage there is often seen a regression to a primitive state of perception. Hanfmann described a patient who was shown a key, which he failed to recognize as an isolated object. But as soon as he saw the key inserted and turned in the lock he smiled and shouted, "key, key, key."

Bender and Teuber described brain damaged Navy men at San Diego who when shown a triangle and a square could not tell the difference of shape. But when they were permitted to trace the forms in the air with a finger the forms were immediately correctly named.

In philology and semantics we are told that the earliest and oldest words in any language are verbs or action-words. Many years ago, Titchener said that "the common denominator of meaning is kinaesthesis." This is undoubtedly why cheiroscopic tracing training properly done does such beneficial things for the reorganization of the perception of form and size-distance relations.

It is most important for us to realize that in the ontogenesis we see also primitation in infancy much the same as in the primitive adults. Werner wrote that "the younger the child, the less purely objective and self-subsistent things become,

and the more highly conditioned in their significance by emotional and motor reactions." Miss Shinn gave her six months old nephew a round rattle instead of his usual square one. The child struggled without success to find and bite the 'corners' of the round one. It was, she said, not an optical or tactual form but rather a 'thing-of-action' understood as a signal for a specific motor-affective set of movements.

Meili and Tobler studied visual apparent movement perception in 38 children ages 5 to 12 years. They found that the younger children could see motion at a lower rate of succession than adults, the turning point being at 12 to 14 years.

This is the age we found, in studies comparing blind and seeing children for cutaneous localization, for the onset of ocular dominance. From about puberty on, vision plays a very different role in perception.

Ward made an extensive study in my laboratory of some 238 children from the second through the 12th grades. He measured very carefully their abilities to judge sizes at various distances from 3 to 96 meters of a standard stereo target 30 by 30 centimeters in actual size. From the second grade at about age 7 through the 12th grade at 17 or 18 years he found no age or sex differences.

Young children show much more of what has been called physiognomic perception than adults. For instance a 5 may be said to be "happy" and a 9 "sad." Mulchow studied this problem and showed that a figure like this:

was named "cruel"; that is a sharp cornered object "behaves cruelly" or "can hurt you." Patterns such as a painting or a young lady's face and figure can be beautiful to some and ugly or repulsive for others. The classification cannot be found in shapes, colors or pure geometric relations of the stimulus object 'out there.'

Finally, we should point out that the untrained perceiver differs from the trained in important and fundamental ways. More about this later.

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1961
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLCGY

March - 1961

Series 21 No. 6

It is not at all strange that primitive man, young children and animals preceived things-out-there as things-of-action. The successful and adequate adaptation to his environs was contingent upon his ability to make responses, by approximation and correction, which yielded the best equilibrium which could be attained to the demand situation.

Each adjustory reaction either did or did not further either his immediate or remote ends. Each left its traces in his body mechanisms. The next time he contronted the same or a similar stimulating situation there would be some recall of the preceding acts and their consequences followed by approach or avoidance. If he had the necessity of many repetitions of the experience he probably did not eat or make love as successfully as his more able competitors. If he was able to profit by the effects of the residual traces of his prior experiences he did well.

If he had a good body and nervous system and effector system he developed many sets, determining and meaning tendencies, attitudes, prodromal conditions, such that the merest glance or sound or odor would touch off the most expedient set of movements, to seek or defend. This is how the end comes to transform and determine the means to the end. By learning that dynamically perception is but an early stage in the ultimate action, good or bad, he learned to see relations. He developed the ability to say to himself that 'this is one of those! His older habits developed less specific patterns and these became less and less dependent upon the specific properties and attributes of the stimulating things-out-there. He became less stimulus-bound. The things-out-there became what they were to him at the moment in terms of the ultimate action systems he built up.

The living organism is a dynamic system. Processes of respiration, heart beat, metabolism and repair go on continuously every minute of the waking and sleeping life. Some of his actions were primitive tropism and taxis. Sleeping he readjusted his posture every few minutes. In behavior there is no such thing as the steady state of physics in which input and output of energy is equal.

Motion is the very essence of life. Without it there is nothing. Perceptual processes eventuate in some form of overt or implicit movement system. The feedbacks from these movements are the potent agents of organization in the perceptual field. They are the least susceptible to adaptation of any nervous processes. They are, as Hoagland pointed out, the pace-setters. They extend the boundaries in behavior of the space-time lattice. They bring it about that perception and action become one.

As the infant grows, develops and differentiates his movements of locomotion, handling, grasping, etc., these leave mnemonic traces in the system. His responses to external stimulation become foreshortened and condensed. Figure and ground organization takes place in the stimulus configurations. Movements for attaining his ends become fewer and simpler as skill accrues. Through the feedbacks the stimulus patterns from the things-out-there become more and more an integral part of the meaning and adjustory movement systems. Perception approaches or reaches the stage of direct apprehension.

Wever showed in his experiments that in the trained individual these processes may

be consummated in as little as .017 seconds. Just as in the well known reaction or voluntary movement experiment the whole course of the 'reaction time' is set in the fore-period, the period of instruction, set, well before the appearance of the 'stimulus.' One reacts therefore not alone to a hypothetical 'stimulus' but the kind of reaction is determined well in advance in time of the appearance of the stimulus energy to a sense organ. Any movement is one which conforms to the plot, schema, frame of reference which is there and ready to go before the appearance of any stimulus.

If a new or strange pattern of stimulation is presented then the stage in the perceptual process known as the search or effort for meaning is engaged. What is it? What will it do to or for me? What must I do about it now? There is a selection or rejection of the projection in space-time of what would result to the organism if a particular one of all the available habit patterns were selected and permitted to operate. This sorting out of the inferences or search for the most appropriate action pattern at the time and under the circumstances is the primitive prototype of thinking.

The meaningful action patterns are there and ready to go, loaded for discharge, well in advance of stimulation or of the linguistic or imaginal equivalents thereof.

Perhaps this is why the late Professor Koffka once pointed out that the most practical things are theories. If, for example, I know the law of tangents and if I can see or relate or identify the new problem to be solved as one which belongs to the class of operations which can give me the right answer more quickly, easily and surely by using the operations of this law, then this fact illustrates that the trained perceiver operates less and less with specific or isolated things. As soon as he sees that this is 'one of those,' that this is a case where he can foreshorten the time and energy to elicit the satisfactory consummatory response, then there is no longer a problem. The rest is clerical work.

The operations which lead to the solution may be either inductive or deductive. The unification of the perceptual-motor processes may or may not be conscious. In an extensive experiment of the psychophysics of the discrimination of the smallest perceptible differences between light-spot sizes our trained observers finally reached a stage of skill in which their absolute judgments were quicker and more accurate by the method of absolute impression than by the method of paired comparisons. In the former case they were shown a standard spot. This was followed by as many as seven test spots, each of which had to be judged as larger or smaller than the standard. The comparisons in this case were with the trace left by the standard.

There is no doubt whatever in the minds of those who have lived and worked with the problem long enough to have earned the right to an opinion that the trained perceiver utilizes a very different set of functions than the untrained, the young child, or the primitive.

For the trained perceiver, skills frame and underlie other skills. Both sensory and motor processes become foreshortened and condensed; become aggregated into more and more general patterns. Tension movements become ballistic movements. In the former the muscle contractions extends throughout the movement and 'runs with the brakes partly on,' that is against some opposition from antagonistic muscle contraction. In the ballistic stage the muscle contraction takes about 1/20 second. The limb or member glides forth in a smooth, rapid and accurate movement, unimpeded by the contractions of antagonists.

The late Professor Raymond Dodge put it this way: "There are... in adult behavior, no pure habitual responses uncontaminated by reaction set and the epicritic elaboration of the sensory data....human adjustment is not a mosaic of conflicting reflexes, habits, instincts and voluntary acts, or a succession of discrete responses under these various categories, but a dynamic continuum....a sort of spiral process with a relatively simple front of overt reaction at any given moment and a highly complex background."

"Each overt reaction is really a complex of approximating beginning reactions and elaborated adjustments. The beginning re-

actions are evoked by current stimuli superposed upon the remains of consummated responses to past stimuli by which they are inhibited, reinforced or qualitatively modified.

The same processes are seen in acts of judgment. In a psychophysical experiment, as Miss Anastasi showed, it is not the same if we compare a variable to a standard or if we compare a standard to a variable, disregarding for the moment the troublesome question as to just what makes one or the other, in perception, a 'variable' or a 'standard.' And, it is well known that the judgment of a difference is much simpler than the judgment of likeness.

Among others, Washburn (1925) and Purdy (1935) proposed a motor basis for the most fundamental of all figure-ground perceptual experiences, the formation of the _unum_ and the _duo_.

For the _one_ (unum) there must be in the visual field a segregated area, relatively homogenous in color, and surrounded by another groundal area different enough to produce a break in stimulation across the contour of the enclosed area. 'Centering' of the segregated or figural area in the field takes place.

Duo formation results when two such contoured areas are present in the visual field, setting up stresses, such as rival fixation impulses, etc. Children see less disjunctively than adults. In both the two light spots of the _phi_ movement become one, moving from one position to the other if the temporal interval between the appearance of the lights is optimal. This is slower for young children than for adults. This accords with the relative stages of perceptual-motor organization. Piaget has elaborated on these facts.

The perceptual-motor integrations which form the sets, attitudes, frames of reference are highly individualized schemata, beyond any question. Each of us lives in his or her own privately created world of perception and action. To the extent that this is so, then one may rightfully ask: What is the meaning of a norm? Is there any such thing as a 'normal' or average person? Normal or average _when_? Normal or average with respect to _what_ in his perceptual-motor world?

Language, communication and social processes create certain stereotypes, it is true. So we can begin to understand why there has been so much confused writing and thinking about such topics as the personality, 'hidden persuaders,' schizophrenia, etc.

Copyright
1961
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

OPTOMETRIC EXTENSION PROGRAM · 1928 · OEP

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

April - 1961 | Series 21 No. 7

In previous papers we have mentioned the changes which have taken place in our understanding of the processes in perceiving which have resulted from the studies of Coghill, Child, Weiss, Lashley and others on the relations of growth, development and differentiation of behavior and of the body organism.

Attention was called to the striking parallel in the similarity of the changes in the culture patterns from the perceivings of the cave man to the present and of the changes from infancy to adulthood in the ontogenesis.

There is no need to emphasize the high importance of keeping aware of the historical perspectives in which any modern theory of our perceptual behaviors is seen or described. And, with this of course goes the necessity of thinking seriously about our real position in the cosmos.

In physics, Faraday and later Maxwell developed the concept of electromagnetic fields. A field was a real thing and not a mere linguistic construct. The distribution of currents in a Wheatstone bridge circuit, for instance, could never be specified no matter how much was known about any single component of the circuit.

Then along came Max Planck. In 1901 he announced his epochal discovery of the law of the radiation of energy, that energy depends upon the frequency of oscillation of electrons. Energy emission, he showed, comes in bursts or pulses. Energy emission was discontinuous; absorption was continuous.

In 1912 he generalized his law, that energy is equal to a constant, h, called Planck's constant, times the frequency of electron oscillation. The value given for h was 6.547 times 10 to the minus 27 power erg-seconds.

Planck received the Nobel prize in physics in 1918. He died at Göttingen on October 4, 1947. The emission of light energy was a matter of bursts or pulses of small quanta of energy.

Then there was Heisenberg who showed that the conventional Newtonian mechanics did not hold in subatomic activities. His 'uncertainty' principle showed that in subatomic activity the possibility of change was not entirely determined by the past.

Schrödinger showed that "in small scale systems there cannot be continuous change, only leaps from one pattern to another." Then came Lisa Meissner and Albert Einstein and the relativity of space-time.

It is enough here merely to point out that the development of quantum physics not only revolutionized physics, but along with it chemistry, biology and psychology. It became evident that "large scale events are funneled through the small scale world of quantum physics, making possible change, growth, evolution and original ideas--all forms of freedom from absolute determinism" (D. Angus).

Look at one consequence of the quantum concept: Genes, said Schrödinger, which had been treated as invariant and elemental determinants of all our physical and mental talents, skills and attributes, "may undergo, because of radiation, a quantum jump resulting in mutation --- so live evolves."

In psychology, Max Wertheimer, about 1912, pointed out that creative thinking, which had proven for centuries a difficult prob-

lem for logic, epistemology and for science, could be treated and studied as a case of closure within the system of fields. Much can be said about this complex process even though most of the process of closure is anteceded by 'silent' processes not available to introspective examination and report.

So, in the first half of this century it is clear that in science something big and important was "in the air"; something which brought man and his behavior out of the pluralistic fog and made him a unit component of the cosmos.

In 1896 John Dewey, then a psychologist, wrote a famous paper in the Psychological Review on the concept of the reflex. In it he showed that the classical concept of the reflex as stimulus-central process-response could no longer be defended or used to tell truthfully 'what happened.' The first term in any reflex, he showed, was not stimulus (which, by the way, I do not believe you can satisfactority define) but postural and accommodative movements to make it possible for the receptors to get stimulated. Stimulation, said he, lies inside, not outside the action.

About this same time our biologist friends developed the concept of the backstroke or feedback and showed that this was the pricipal pace-setting, organizing agent, with its comparative freedom from adaptation effects and its consequent expansion of the space-time lattice. Motor processes can and do transform the sensory inputs. Often the stage is set for some particular effector set of processes well in advance of any external stimuation.

A newer and different concept of perception and action was in the making. The notion that habits, for example, were fixed systems of conduction along invariant telephone-telegraph like lines in the brain and nervous system could not be defended and made to fit the facts both of experiment and of quantum theory.

Clearly here, as in perception, the trend was toward unity. Man was something in and of nature and subject to its laws like everything else in the cosmos. Quantum physics has shown that all events, from the simplest to the most complex, form a single unitary system. This position has been ably expounded by John Dewey and Arthur F. Bentley in a book published in 1949 entitled Knowing and the Known, (Beacon Press, Boston).

In this book they consider self-action, in which things are viewed as acting under their own powers, more than less independently. Interaction constituted the case where 'thing is balanced against thing in causal interconnection.' And their principal thesis deals with transaction in which systems, measurements, descriptions and namings are "employed to deal with aspects or phases of action without final attribution to 'elements' or other presumptively detachable or independent entities, essences, or realities, and without isolation of presumptively detachable relations from such detachable elements."

Transactionally the organism-as-a-whole is viewed and described as the organism-in-environment-as-a-whole. This was Professor Ronchi's objection to physical and physiological optics. Or the quantum position as to Newtonian mechanics. They can be true and acceptable only if their limitations are specified. They deal with an artifact model "world with man left out".

The transaction approach and methodology makes no differentiation of subject and object, mind and matter, soul and body, self and not-self. Dewey and Bentley held that "any statement that is or can be made about a knower, self, mind or subject, or about a known thing, an object or a cosmos... must...be made on a basis and in terms or aspects of events which inquiry, as itself a cosmic event, finds taking place."

"Transaction assumes no pre-knowledge of either organism or environment alone as adequate,..... but requires their primary acceptance in the common system, with full freedom reserved for their developing examination."

These authors hold that "mind, faculty, I.Q., or what not as an actor in charge of behavior is a charlatan, and 'brain' as a substitute for such a mind is worse. Such words insert a name in place of a problem...The living, behaving, knowing organism is present. To add a 'mind' to

him is to try to double him up. It is double-talk; and double-talk doubles no facts."

They state that "despite all the fine physiological work that has been done, behavioral discussions of vision in terms of images of one kind or another, are in about as primitive a state as they were a hundred years ago."

In their work on perception, Ames, Cantril and Ittleson put the transaction matter this way: neither perception nor thing perceived exist independent of the total life-situation of which both are subparts. Transaction is used to designate such a case and implies that all subparts of any situation enter into it as active participants, thus determining as a resultant the behavior vectors. The 'parts' owe their real existence as seen in the situation to this active participation or co-action. They are not existing entities which merely interact with one another without affecting or changing their own identities.

This is enough to show that scholars are busily engaged in serious efforts to examine and interpret the operations we call perceiving, learning, remembering, thinking, within the framework of the best and most modern scientific postulates. There is but one science, which is a better way of describing and interpreting the things and processes in nature. The 'divisions' of science which we call physics, chemistry, biology, psychology, etc., are man-made and artificial. But they are necessary because of many limitations of time, interests, etc. They impose upon us the burden of the necessity of patient understanding and cooperation in the search for truth, which makes us free.

Copyright
1961
OEP Foundotion Inc.
(Nonprofit)
Duncon, Oklo.

OPTOMETRIC EXTENSION PROGRAM
OEP
1928

VISUAL PSYCHOLOGY

Studies from the loborotory of Somuel Renshow, Ph.D., professor of experimentol psychology ot Ohio Stote University, supplying optometrists with voluable procedures for use in the proctice of functionol optometry.

VISUAL PSYCHOLOGY

May - 1961

Series 21 No. 8

In the two preceding papers we have attempted to show that greatly important changes have taken place in the first half of the present century in our basic postulates as to man, his behavior, and his place in the cosmos.

The discovery and development of quantum theory in physics has shown that what we call things are aggregations of something like thirty-two kinds of extremely small masses called electrons, protons, mesons, neutrons, etc., orbiting at prodigious rates about a nucleus.

Energy, said Einstein, is mass times a constant squared. The constant is the speed of light. But light is a series of pulses or bursts of energy. If the sine waves are between 400 and 700 millimicrons in length our retinas absorb some of this energy and transform it into another kind of energy. This in turn moves on in a most complex system of virtual transfers to produce a different form of energy release and transformation in our effector organs.

These in turn send feedback bursts of weak energy into the system in a closed loop circuit. The organism acts, moves, perceives, remembers, thinks, gets ready for the next series of events of approach, avoidance, etc. This is organic behavior.

Dewey and Bentley, considering behavior, looked at it in the light of the consequences of quantum considerations. In such a system, they concluded that the division of the cosmos into 'things out there' and the person or perceiver 'in here,' that is within his skin, could not be held or defended.

Percepts were not responses to things as stimulus objects out there. The life sustaining air was not only out there but it was also in the lungs, the bloodstream, the tissues and organs. The environment was both inside and outside.

These men rejected concepts of self-action, that is of things acting independently by their own powers and often contrary to probabalistic considerations.

They also rejected the concepts of interaction, in which things act with or against other things in some sort of causal relationship. In such systems, the search for the ultimate first 'cause' of any action merely went back and back and back and became lost in a hopeless teleology, the anti-science.

The way out, they proposed, was transaction. They held that man and his behavior is one with all nature. Perception and things perceived are acted upon as designated sub-parts of the total. Really there are no 'parts.' Parts are only artificial or man-made limits set by the constraints or boundaries of segregation or aggregation within the system. Only a few degrees eccentric to the foveas and the light patterns and their transforms are blurred and formless. Parts are thus a kind of local action in the organism-environment unity.

The cell for more than a hundred years after its discovery by Schleiden and Schwann was regarded as the basic unit of life. "Today there are only limited regions of physiological report in which the cell retains any such status." The cell is what it is and does what it can do within the specifications of its electrochemical environs.

To quote, Bentley and Dewey say that "the anticipated future development of trans-

dermally transactional treatment (of perceiving and behavior) has been forecast by the descriptive spade-work of the ecologies, which have already gone far enough to speak freely of the evolution of the habitat of an organism as well as the evolution of the organism itself."

Early in World War II, I talked with a fine young Navy man at a base in Connecticut. When I asked how he liked it here, his answer was an emphatic NO. When I asked why, what's the matter, his reply was, "It's too crowded. Town's too close together; no elbow room." Then I asked where are you from? The reply: "I have always lived and grown up on a ranch about 30 miles from Missoula, Montana."

At the time when the principles of the transactional viewpoint was being developed by Dewey and Bentley, and later by the late Adelbert Ames and his co-workers, there was another closely similar viewpoint 'in the air.'

Many years before this had been proposed by several German and Austrian philosophers of science: von Ehrenfels, Husserl, Meinong and by Ernst Mach. It was also in process from the work of Max Wertheimer and Wolfgang Köhler in psychology. In 1920 Köhler wrote a brilliant exposition in a little book <u>Die Physische Gestalten</u>. It was and is the principle of <u>isomorphism</u>.

Mach had published in 1880 his notable <u>Contributions to the Analysis of Sensations</u>. In it he asked: What are spatial forms? Melodies? Are they a mere assembly of parts or elements; or, are they something new accompanying and emerging from the aggregation, but none the less distinguishable from them?

Ehrenfels held that melodies, totals, were different entities than the mere sum of their parts. When a melody is reproduced in a different key the whole is the same; the parts or notes are not. This he called form-quality.

Dilthey, another philosopher, said that "the whole is more than the sum of its parts" and that the apprehension of the whole is a condition precedent to the adequate interpretation of the 'item' or part.

Köhler showed in his book that physics is really a <u>molar</u> science. For example, water he said is composed of two gases, hydrogen and oxygen, combined in such a way that each molecule is composed of three atoms, two of hydrogen and one of oxygen. Hydrogen occurs in nature only as hydrogen molecules, each composed of two hydrogen atoms. We thus have H, H2, and H2O, each with different properties, which cannot be derived by adding the properties of the H's and the O's. Water is wet, for example, and this could not be predicted from no matter how much one knew in advance about hydrogen and oxygen.

The principle of <u>isomorphism</u> is, if anything, older than quantum physics. This principle holds merely that there are <u>molecular</u> processes in nature; and that there are also <u>molar</u> processes in nature, like the sequence of separate tones which make a melody. Isomorphism says merely that molecular processes and molar processes are not two different things; that they are stages or phases of a single reality; that molar processes are molecular processes <u>in extension</u>; extension in space-time.

If you sound two tones, one of 256 cycles and one of 384 cycles per second simultaneously, you will hear several tones, called Tartini tones, which are all the audible sums and differences of the two frequencies. But it is easy to show that there are only two frequencies in the air. Where do all these other tones come from? They are contributed by the hearer. They are not 'out there' in the stimulus. Many years ago Titchener said that we always perceive more or less than is furnished by the stimulus.

These were some of the arguments and evidences offered in support of 'wholism,' the organismal hypothesis, physical monism, transactionism. But then, as now, there were and are the dissenters.

How, it was asked, can one measure study and describe the covariant changes in the whole organism-environment unity? If everything that has ever happened is the determinant of what has just happened, how far back must one go to reach the point of no measurable effect? How can

you describe or communicate facts about the room in which you are right now unless you analyze or break it up into its constituent parts?

Questions like these need not thwart or embarrass the transactionist. There is nothing wrong, he says, with the measurement or description of molecular or part processes. The wrong comes from omitting important parameters in the descriptive and interpretative equations. Because of human, temporal, spatial and mechanical limitations science is forced to use reductionism. You can measure brightness, contrast, color, etc., in an angle of a degree or two in the fovea, with the periphery blacked out or 'held constant.' But you must be very cautious not to extrapolate. The facts you get are only true under the reduced conditions of your measurements. They tell nothing about what happens in the summative way with the normal or naturally active periphery. How much does one distort things and processes in reduction? Is a law of the single variable attainable?

There are ways of handling the theoretical and practical difficulties indicated briefly above. Some of these we shall consider next time.

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1961
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

June - 1961 Series 21 No. 9

In the preceding paper of this series we pointed out that the advent of the concepts of quantum physics compelled a radical and far reaching change in basic thinking. This went for all divisions of science. It was established beyond question that the whole of nature is comprised of swirling masses of extremely small entities, some thirty odd kinds of them, moving orbitally about nuclei at very prodigious rates of speed.

In organic nature studies of the relations of the developing nervous system and behavior showed that the foundation principle was that of metabolism. This entailed movement and locomotion and ultimately led to the development of ways of perceiving and differentially reacting to space, form, motion, brightness, temperatures, postures, etc.

We attempted to point out that scholars, looking at the problems of why things look as they do, had long since become dissatisfied with atomism, with the over-simplicity of elements, of associations between these assumed realities, of stimulus-response doctrines, conditioning, reinforcement, and the like. Cells were what they were because of where they were as well as what they were.

These things simply could not be reconciled with the concepts of organisms, of field dynamics and of the internal and external environs and the interdependencies arising therefrom.

We discussed briefly the transaction proposal of Dewey and Bentley as one possible example of a way out. Perhaps a better designation may have been co-action to banish more completely the implied dualism of the agent, the perceiver and actor, and the 'thing out there' acted upon.

The late Adelbert Ames and his co-workers faced this problem. The traditional question was: Assuming the reality of the physical world 'out there,' what is the related or covariant perception? Ames said let us look at the problem the other way round: Given the perception what is the related thing or set of things in the physical world?

Ames' group studied extensively aniseikonic people. He saw that people with fairly large amounts of aniseikonia could see normally quite effectively and often unaware of any perceptual errors, merely reporting headaches, stresses in seeing, etc.

When fitted with appropriate lenses and wearing the same lenses he would perceive different distortions depending on what he was looking at.

From the geometric optics of the situation he found that he could not predict the person's perceptual experience. If he looked at a lawn, for instance, it appeared tilted. But if he looked through the same lenses at a lake, then this would appear to him more distant but in the proper horizontal position.

The group searched for 'cues,' for binocular stereoscopic conditions, for accommodation-convergence relations to account for the visual space and form. Regardless of the presence or absence of their accomplishments the question remains: Were they not, in so doing, abandoning transaction postulations and actually using the postulations and methodologies of traditional elementarism?

If one sets about to answer the questions: How does any person see his visual world? What are his deficiencies? What must be

done to improve them? We then proceed to test and measure his visual functions. We give discrete names to many of these and more or less tacitly assume that each measured function is an independent thing or process. Often we nomenalize then reify.

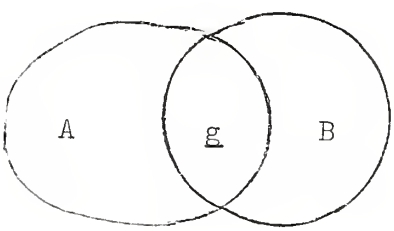
The same person in reporting or describing what he sees uses his own highly individualized frames of reference, schemata, memory traces, sets, etc.

If two functions, A and B, are measured we have to face the question as to how much of A is B and how much of B is A?

A and B can only be invariant and independent processes if they are the same. That this is rarely if ever true may be shown from abundant evidence that the inputs from the sense organs have to be processes by the same agent, the perceiver, whose anteceedents, sets, sentiments, physiognomic characters have a common term in the equation of everything he does.

Back in 1904, C. Spearman published a paper in the American Journal of Psychology, Vol. 15, 201-293, entitled "General Intelligence, Objectively Determined and Measured." This contained his proposal of momentous importance, his famous two factor theory.

Spearman assumed that any correlation between two tests of 'intelligence' implies a factor common to both plus two specific factors. This may be represented as:



The common factor is g and the specific factors are A and B. 'g' was something common to both A and B. It either could or could not be named or recognized.

It is easy to see that if one has ten such measures the intercorrelations would show how much of each includes how much of the general factor g, common to them all. This was an important forward step.

But Spearman was not the first to think along these lines. In 1854 George Boole, an English logician and mathematician, published an important book on The Laws of Thought. In it he laid down the principles of what is now known as Boolean algebra. Suppose, for example, you have four functions measured, A, B, C, and D. Boolean algebra is a means of computing <u>how much</u> of each is or is not included in all the others.

These things were prominent forerunners of modern factor analysis. I suggest that you will know about it if you will and can read J.P. Guilford's Chapter 16 in his <u>Psychometric Methods</u>, pp. 470-538. Factor analysis is a rather complex mathematical process for discovering the actual constituents of the things or functions we give names to.

For example, during World War II, my colleague, Dr. R.J. Wherry took the Ortorater and Sightscreener results on about a thousand military personnel. He did a factor analysis of the several tests. In his published government report he showed that about 65% of what is called visual acuity is really form perception.

Back in 1948, F.N. Jones published a paper in the American Journal of Psychology on a Factor Analysis of Visibility Data. He took the spectrophotometric relative sensitivities to wave lengths of 493 to 678 millimicrons measured by Coblentz and Emerson at the National Bureau of Standards in 1918. Jones selected 20 of the wave lengths within the range noted and computed all the intercorrelations among them. These he set in a correlation matrix. He then made a centroid analysis and followed this with five rotations with all axes orthogonal and obtained the factor loading for each variable. This gave him a transformation matrix. From this he derived <u>three</u> factors. These he found agreed almost perfectly with the three primaries which Deane B. Judd published in the J. Opt. Soc. America in 1945.

It is not possible in a brief paper to do more than indicate briefly the fact

that by using factor analysis and similar devices it is possible to analyze out of the complex functions of the self how much of our perception and judgment of the position, size and distance of an object is contributed to by the things we name as acuity, brightness, form, contrast, phorias, stereopsis, figure processes, ground processes, the constancies, eye-hand coordination, etc. One of our present research studies is aimed at this sort of thing with school children as subjects.

But a word of caution. Even if you learn from books on statistics how to do a factor analysis you must first be able to answer the question as to whether you can legitimately compute the zero order correlations among your variables. Some of them at least are non-parametric.

For example, in spite of the fact that figure alternation has been shown to be an essential factor in stereopsis, and that it is a function which is discontinuous at <u>two</u> points, one young West coast man recently correlated the alternation rate, which is <u>not</u> a Gaussian distribution, with the point hour ratios of some forty engineering students and found no relation. What he did was wrong no matter what he got. Sooner or later someone will set the record straight but meanwhile his paper will be quoted as fact, by the uncritical ones.

Our contention here is that it is possible to know what and how much each of several functions contribute to the complex sets of perceptual acts of the whole organism; that such facts are very incompletely known at the present time; that much of such work from this point of view must be done in the years ahead. If we are strictly honest with ourselves some at least of what we are doing now is heavily loaded with guesses, some good and some decidedly otherwise. Grouping, group theory and analysis is relatively new as a mathematical analytic tool. Like the English language it is "an instrument of precision. Learn to use it wisely and well."

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1961
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshaw, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

July - 1961 Series 21 No. 10

In our last paper we made brief mention of the fact that there are mathematical ways of taking a number of measures of various visual functions and determining what the common factors are which are partly included in each.

These factors often cannot be given a name since we cannot in the present state of knowledge measure or describe them as separate, localized or bounded entities except by indirect methods.

We pointed to factor analysis as one powerful and important means of such analysis. Its use and servicability depends upon the answer to whether or not the products-moments coefficients of correlation, the basic raw materials for factor analysis, can be computed legitimately. That is to say that it must be shown first that measures of each function used in the study are distributed in a 'normal' or Gaussian curve or dispersion. A Gaussian distribution is the well known bell-shaped curve.

If you take a pair of dice and cast them a thousand times you should get such a frequency distribution. Each throw can give you any number from 2 to 12, sometimes called 'snake eyes' and 'box cars.' If the results are plotted the number which is the mean, median and mode of the curve, that is the number which came up most frequently is 7. This is simply because there are more combination of the dice which add to 7 than any other number.

If one takes the frequencies for each of the 5 numbers on both sides of the 7, one can compute the root mean square 'error' or standard deviation of the distribution. It will be found that within the range of six such units laid off on the x-axis all but about 2% of the values will be included. In the case of the dice it will include all of the cases because you cannot get any number less than 2 or more than 12.

One can now take a table printed in every good statistics book and determine the probability, or betting odds, that if you throw a 9 that another 9 will show before you get a 7. The gambling impressarios get rich because so many people think they can get lucky and disregard the laws of probability or because they know little or nothing about 'chance.'

The premium you pay for each thousand dollars of life or accident insurance is computed in the mathematical probability way. The company bets you $1,000.00 against your _N_ dollars premium that you will be alive on your next birthday.

A Gauss distribution is a measure of something which follows the probability principle. But what about visual functions? Are they Gaussian? Can we correlate and factor analyze the measures we make of them? If not then one must use non-parametric mathematical analyses.

In a study of 94 fourth, fifth and sixth grade children, now in its third follow-up year, Mr. Bruce A. Rupp completed a Master's thesis in 1959 here in the laboratory to answer questions like those above. Which of the dozen or so functions measured are 'normal' or Gaussian functions? How well can one predict academic achievement from the battery of visual functions measured?

Mr. Rupp could and did test for 'normalcy' by plotting each function on probability scale paper. This scale is made such that if cumulative frequency in percent is plotted on the abscissa or x axis and the function to be tested for normalcy is

plotted on Y, the locus of points plotted to each X and Y value will fall on an approximately straight line.

But if the points plotted locate a curve, or a compound curve or if they yield two distinct straight line equations the measures may NOT be considered to approximate the 'normal' or Gaussian distribution and so correlations amongst them or with a criterion such as school grades could only give information that is misleading, untrue and meaningless.

The general slope of the straight line through the points on X and Y must be positive. The magnitude of the slope is determined by the size of the Y axis scale intervals. Are these intervals arithmetic or geometric?

Here we are confronted with a real problem. How shall we scale a near-point phoria? Some might say "That's easy...... just set off on Y the number of measured prism diopters." But wait. How was the function measured? Is the near exo. an independent entity or is it what it is because of some thing or things other than just phoria, a word whose Greek origin means bearing, position and direction?

Rupp showed that, of the dozen or so measures on each of the 94 children, truncated into high and low achieving groups, several measures were not normally distributed or Gaussian. For example, some of these were what we are wont to call parallax threshold at near, size-constancy index at near, lateral phoria at near, and figure alternation. Macular fusion amplitudes at near gave a normal distribution.

It would be important for us to know just what the common factors are between parallax threshold or stereopsis, size-distance judgments, lateral phoria and macular fusion amplitudes. But it is clear that the answers to our questions cannot be obtained by the usual methods of correlational analysis.

It seems clear that we have to think of visual functions of form, size, position, distance, motion, etc., in terms of geometries of fields or spaces which are non-Euclidean. Think of the famous alley experiments of Hillebrandt and of Blumenfeld. To set a row of small lights which are parallel to binocular vision on the Z axis the lights must be separated outward more and more proportional to the distance from the observer.

Helmholtz long before looked at a line of small point sources horizontal to the line of sight. When he fixated the middle of the line the lights appeared curved concavely, and when he changed focus a little beyond the line the lights appeared to be curved convexly.

These and similar observations led the late Rudolf Luneberg to attempt to discover the proper geometry for the measurement and description of binocular space. He came to the conclusion that one must use an hyperbolic geometry. His publication was A Mathematical Analysis of Binocular Vision, Princeton Univ. Press, 1947.

If you are interested in the problem I suggest that after reading Luneberg's monograph read the paper by Thorne Shipley on The Convergence Function in Binocular Visual Space, Journal, Opt. Soc. American, 1957, 47, 795-821.

It has been our good fortune by using what the mathematical experts have assured us is a valid procedure to show that our battery of visual functions measures gives a positive coefficient of relation of 0.83 with scholastic achievement, measured by standardized achievement tests in the schools. This can be interpreted to mean that not less than 69% of the variance in achievement was associated with the variance on the visual functions.

Good vision tends strongly to go along with good scholastic achievement; poor visual skills with poor work.

We hope to be able to sort out by non-parametric analysis how much of this is that and how much of that is this. If this can be done we shall be in far better position to develop more effective measuring and training devices.

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1961
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

August - 1961

Series 21 No. 11

In previous papers of the series we had occasion to make brief mention of the principle of isomorphism. This has to do with the relations of molecular and submolecular processes in living cells, the tremendously complex electrochemical events in metabolism and reactance, and the molar processes of perceiving, remembering and thinking. Isomorphism merely holds that these processes, designated as molecular and molar, are not two independent things but merely phases in the unitary series of events and operations broadly designated as living behavior. Molar processes are regarded as molecular processes in extension; extension in space and time.

For the primitive and the untrained the organism or self is <u>in here</u>, and things are out there. But even on casual reflection it is soon seen that the life sustaining air is not only out there all about us but it is also in here in every cell. It has been shown recently that the free ions in the air we breathe have much to do with our thresholds of sensitivity to light, sound, pressure, temperature, etc. We may be quite unaware of our position in the gravitational field until we meet someone with Maniere's disease or certain instances of aniseikonia.

It must be clear that the organism is not an independent entity set off from its surrounds by the constraints of the skin.

Modern science has come a long way in bringing us to a better understanding of how our organisms operate consequent to some resolution of the welter of determining forces in the total field in which we live. Recall that only 69 years have passed since the discovery that the nervous system is made up of myriads of cells called neurons.

It is interesting to compare the thoughts and beliefs of people before this time and now. Nothing, to my knowledge, in nature is more wonderful than the brain. It is composed of something like ten thousand million nerve cells. Not long ago I was privileged to look at a minuscule bit of a living retinal rod magnified some 375,000 diameters in an electron microscope. One such look should convince the most skeptical person that the electrochemical processes going on constantly in this bit of living cell are clear manifestations of dynamic, interdependent mechanisms. Each complex molecule gives and takes, changes size, position, operations always in an amazing system of law and order. Some processes only take place in the presence of a catalyst. What it is and does depends on its electrochemical relations both with its immediate and remote surrounds.

When the energy we call light comes in successive bursts or pulses to a photosensitive cell, any one of about eight structural types comprising a retina, the energy becomes transformed and is continuous for a brief time, then is changed again into the relatively weak and slow moving sinusoidal pulses we call nerve currents.

The physical chemists show us that the propagation of energy in a nerve conductor is a virtual transference of ions; that nothing ever really flows over or through a dendrite or a neurite. It is like the 'flow' of current in a copper wire or like a row of billiard balls in apposition where one other ball approaches and

strikes and end ball head-on and the one on the other end moves off the line.

We have pointed out that the work of many able scholars, like Coghill, Child, Weiss, Lashley and others, has shown that the concepts of the workings of the sense organs, the brain and the effectors as a telephone or telegraph set of circuits can no longer be defended.

The same effector processes of perceiving, learning and remembering may be and most likely are mediated by constellations of perhaps millions of active cells in which the total pattern of energy distribution and propagation is most probably never twice indentical.

Think, for example, of J.F. Fulton's rotational principle, that in hundreds of simple flexor movements of a finger, the same pattern of contracting cells is not repeated. Or of the work of the late Professor R.H. Stetson and his colleagues in their analyses of skilled movements. Or of our own studies on learning to operate a pursuitmeter. When the pattern of transit was reversed there was no change in time or errors, even though the operators had now to make an entirely different set of successive pursuit movements.

These facts all fit the concept that behavior serviceable to the organism is flexible. Man is not a passive, nickle-in-the-slot machine.

The conscious direction of attention can produce figure effects in the absence of 'objective' external stimulation. Consider, for example, the well known Schumann effect. An observer fixates monocularly a small point source of light in a homogeneous dark field. He is instructed to maintain fixation but to attend a point say 30 degrees temporalward to the right eye and 30 degrees upward in the field for the appearance of a second light point. After a few seconds the observers reported diplopia. They say a second spot at the point they attended. Just as in the phi effect where two become one under proper conditions of timing, etc., so in the Schumann effect it is not surprising that one can become two when the mechanism is set to react that way.

In the brain field it is well known that the spread of the energy of excitation is quite diffuse. Recall the discovery of van't Heuven about 1925 that the spatial distribution of energies in foveas and periphery are reversed in Brodemann's areas 17, 18 and 19.

In recent years Köhler and his co-workers have shown that when the eye is stimulated by a slowly moving light across its field with central fixation the alpha waves in the EEG recording are deflected toward the anode. These are direct current effects. They pass through millions of cells, the brain envelopes, the skull and scalp to the electrodes. These effects have been known for years to the physiologists as anelectrotonic effects. They are postulated to be the important mechanisms in figure formation and the emergence of figure from its grounds. The high stability of figural after effect phenomena, as Professor Michael Wertheimer has shown, cannot be effects of reverberatory circuits of alternating currents.

Polarization effects are very interesting and important. It has long been known that hystersis effects can and do take place in protoplasm in direct current fields. These effects can be transitory or more or less stable, depending on the constrain forces operating in regions of the field adjacent. Such figure processes as the summative positive after-images of long duration (P.F. Swindle images) are probably examples of the stabler types.

The eminent Linus Pauling has recently shown in a brilliant paper (Science, 1961, 134, 15-21) entitled "A Molecular Theory of General Anesthesia" that "consciousness and ephemeral memory (reverberating memory) involve electric oscillations in the brain, and that permanent memory involves a material pattern." Such a material pattern can be anelectrotonic effects or hysteresis effects. Pauling proposed that certain anesthetic effects (chemical) which bring unconsciousness are produced by agents which "form hydrate crystals in the synaptic regions

of the brain and from the resultant increase an impedance of the neural network and correspondingly decrease the energy of the electric oscillations" and so bring on unconsciousness. This is a process of chemical cooling. Cooling to 27 degress centigrade brings narcosis. A drop of about 6 to 8 degrees thus produces anesthesia in the brain. Hi

Hibernation, he says, "may similarly involve the induction of unconsciousness by the formation of hydrate crystals on decrease in temperature."

To do its work the brain needs and must have, among other things, several types of enzymes, a sufficient supply of oxygen, a constant and correct temperature, inputs from the sense organs (think of the isolation experiments) and effectors capable of coordinated movements and of supplying the all-important pace-setting, integrating functions of the feedback inputs. These are well known facts. They point up the necessity of thinking and realizing that such complex functions as the perceiving of form, space, time, motion, size, position, distance, etc., are the conscious correlates of basic biochemical and electrochemical processes. Both the physical and psychological events are stages or aspects of one basic and fundamental reality in nature. To introduce a dualism only brings confusion and misunderstanding.

This discussion, it is hoped, may stimulate the reader to strive for a more complete and comprehensive concept of what is meant by a field and its manifold operations.

VOLUME 33 — OPTOMETRIC EXTENSION PROGRAM PAPERS — (POSTGRADUATE)

Copyright
1961
OEP Foundation Inc.
(Nonprofit)
Duncan, Okla.

VISUAL PSYCHOLOGY

Studies from the laboratory of Samuel Renshow, Ph.D., professor of experimental psychology at Ohio State University, supplying optometrists with valuable procedures for use in the practice of functional optometry.

VISUAL PSYCHOLOGY

September - 1961 Series 21 No. 12

One objective of this series of papers has been to review in brief summary some of the important facts about the relations of the nervous system to behavior. The functions of the brain in perceiving and remembering still constitute 'the dark continent' in many respects. Yet there are many facts which can be structured into a working concept of field dynamics leading to a more complete understanding of the processes in perception. It is suggested that the eleven previous papers of the series be re-read before going on with this one. This should help to put things together.

We have used the term isomorphism in its biological and psychological meaning, that is in the identification of molar and molecular processes. The term is really a mathematical one originally. The late Eric T. Bell put it this way: if two 'things' have the same 'form' they are said to be isomorphic. Mathematical isomorphism may be either simple or multiple.

In the simple case suppose I wish to drive from Columbus to Chicago by way of Fort Wayne. I look at a good road map. It tells me that to get to Chicago on route 30 you pass through Fort Wayne. The <u>real</u> towns and the <u>real</u> road are the points and lines representing the road on the map. The things mapped are identified with the map.

Isomorphism relates to the 'mapping' of other things beside roads and towns. Any scatter plot of two measured variables. <u>y</u> and <u>x</u>, is a picture of the relation of the two things or functions. We need not be limited to <u>two</u>, but may work with <u>n</u> measures or variables. The map is not the thing mapped. It is an <u>as if</u> matter.

No map can be complete enough to show all the houses, trees, pebbles, dogs, etc., along the way. We can and must with caution interpolate and extrapolate, which means we can make astute guesses beyond the map. We can abstract and generalize or idealize from the 'givens' of a good 'map.' One mathematician put it this way: "Interpolation is the mother of prediction; extrapolation the father or prophesy."

Much of our understanding of the nervous system and its role in behavior has come from 'maps.' This obviously has had to be the case. For example, E.D. Adrian has pointed out that all afferent nerve impulses are of one kind regardless of the sense organ from which the transformed energy originates. They differ only in wave length, frequency, wave form and amplitude. There are only about 600 frequencies. These in some manner have to account for all the manifold perceptual discriminations we can make in all the sense modalities. Obviously identity and invariance is not the answer.

Lashley showed that central integration "establishes fields of force to determine spatial orientation and the control of the serial timing of activities." His trained animal subjects could and did make visual discriminations after surgical removal of all but 2% of the visual cortical cells. I often wonder what the engineer, the information theorist and the cyberneticits are talking about when they discuss 'bits' of input information. If they mean that the organism is an analogue and not a digital 'computer' then well and good. Titchener one time said that definitions would be fine if we did not have to use words to make them.

The steady state of physics does not exist in living organisms. Cells are continuously active as long as they are alive, attested by the facts of kinesis and ethology. The organism does not react from a null state. It is always implicitly set to go

somewhere, do something. Some groups of effectors are more 'ready' at any moment to respond than others. These 'sets' or heightened tonus states are always there well in advance of stimulation. Dewey saw this more than a half century ago when he showed that in the classical reflex the stimulus always lies inside and not outside the act.

In the classical reaction-time and complication experiments it has been shown that the things in the foreperiod, the period of set, instruction, intent, are the real determinants of the kind of reaction. The stimulus merely triggers off mechanisms which are active implicitly before the sense organ receives the excitatory energy.

In field dynamics the fact of closure means that often we need but a small part of a stimulus pattern to redintegrate the whole. Lashley showed that "the same neurons which retain the memory traces on one experience must also participate in countless other activities." Regions which are actively compatible or 'similar' tend to form a dynamic unity. Such homogenous grouping of functions may be regarded as the perceptual frames of reference, the schemata of Bartlett, the sets, attitudes, determining and meaning 'tendencies' actively ongoing well before any external stimulation.

Stimulation is always katabolic. It upsets a state of relative equilibrium. Reactance is always anabolic, constructive, restorative of the best state of equilibrium or homeostasis which conditions will permit.

Through the facts of perceptual integration 'parts' can trigger off the same consummatory responses as if the whole were present. We do not have or need molecular identities in stimulus patterns to do this. If I set up my camera on a rigid tripod and photograph a stationary object and its surrounds, I take 5 exposures on 5 sheets of high resolution film. I develop these identically by time and temperature controls in the same chemicals. I fix, wash and dry.

Suppose now I examine an identical small region of each negative under a high magnification microscope. The silver molecules, blackened in the developer, aggregate into clumps. No two are alike in spatial distribution. The five prints look identical in perception. The actual stimuli are all different.

Similarly five looks at the same object by a stationary eye cannot conceivably produce five identical patterns of retinal excitations. The photoreceptors that are in excitatory phase are not the same at any two brief instances in time. Some fraction of the 120 million rods and 9 million cones fire and when the afferent impulses reach layer five the amacrine cells spread the energies laterally, then in something like avalanche conduction the impulses feed through a much smaller number of Betz cells and then on to the much smaller number of afferent conducting fibers in the optic bundles.

McCulloch has shown that about 30% of the energy then goes to the lateral geniculates, superior colliculi, and to corpus striatum and becomes modulated and correlated with afferent volleys from all the other active receptor organs. Some 10% of the incoming energy goes to the reticulum and on at once to the cerebellum. Energy reaches area 2 and 8 about the same time it reaches areas 17, 18 and 19. The spread of energy in the cortex is through widespread areas and in so doing, as Köhler and others have shown, produces polarization or anelectrotonic effects--the stabler forms of energy which are the paradigms of figure processes which emerge from the surrounding grounds of attracting and repelling forces of organization.

We must not lose sight of the fact that things perceived are things-of-action. Conation is as essential a constituent of perception as is cognition. Stop the motor processes, and their pace setting and organizing feedbacks, and there is no consciousness. Skilled movements frame and underlie all acts of perceiving, naming, approach or avoidance and so set the stage for reorganization of the patterns in terms of their consequences for the organism.

It can not be questioned that the principles of field organization in perception and action, the operations we call prox-

imity, similarity, closure, good continuation, precision, etc., are merely statements of the events which take place in the sensory-cerebro-motor-feedback system. Perceiving is one of the components in the Sherrington concept of the integrative function of the nervous system.

Recently one of my graduate students, Mr. Edgar Northup, completed a master's study of the phi effect. He showed his observers in proper alternation two pendulum figures which appeared as a single pendulum swinging back and forth in a horizontal plane. Almost all observers noted the well known phenomenon that if left and right regions of the field are not equally sensitive to brightnesses the motion pattern becomes conical. This is known as the Pulfrich effect.

When Northup gradually increased the rate of alternation his subjects showed positive practice effects. They could see smooth motion at significantly greater speeds of alternation than they could at the beginning of the experiments. The phi-effect can be trained.

He measured a number of visual functions before and after securing the increased speeds from practice effects and found statistically reliable increases in several of the functions measured. This is a species of what I have called sequential training. Bring up some things and others come along too without doing anything directly about them.

It would seem therefore that named visual functions are not independent and invariant entities. Some of this is that, and some of that is this. It is true that we must often use reductionism to measure and describe some facet or dimension of the organic unity. But we must also strive to avoid the pitfalls of elementarism and wrong interpretations.

For an interesting diversion sit down and make a list of the concepts in vision which are widely accepted as facts, but which in the modern day are known to be untenable and untrue. Try to rigorously define words like stimulus, retinal image, perception, illusion, acuity, stereopsis, phoria, etc.

Obviously there is much work to be done to bring all the divisions of visual science to a common basis of theory, operations and communication.